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JOB CREATION AND THE ECONOMIC STIMULUS PACKAGE

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TABLE OF CONTENTS

1.0 INTRODUCTION

In response to the economic downturn of 1974, a series of federal programs has emerged to provide short-term stimulus to lagging local economies. The Emergency Jobs and Unemployment Assistance Act of December, 1974 authorized Title VI of CETA, created the Job Opportunities (Title X) Program of EDA and extended state unemployment insurance and federal Supplemental Benefits by up to 26 weeks of additional federally-financed Special Unemployment Assistance (SUA). A continuing sluggish economy encouraged Congress to legislate additional counterrecessionary measures. The Local Public Works Capital Development and Investment Act of 1976 authorized $2 billion for public works projects in selected local areas. Most recently, the Economic Stimulus Appropriation Act of 1977 created a two-year $21.1 billion package designed to reduce unemployment and quicken economic recovery.

The key features of this Economic Stimulus Package (ESP) are: (1) a $4 billion increase in the Local Public Works program (LPW) administered by EDA; (2) a $2.25 billion appropriation for Anti-Recessionary Fiscal Assistance (ARFA) from the Treasury Department to state and local governments; and (3) a $6.847 billion expansion of DOL's Public Service Employment program (PSE) under CETA Titles II and VI. The Local Public Works program is funding local capital improvement projects, such as dams, roads, public buildings, sewers and repair and maintenance of public facilities. LPW funds are intended to be concentrated in cities and "pockets of poverty" with particularly high unemployment. Anti-Recessionary Fiscal Assistance grants are also concentrated in areas of high unemployment, allocated according to a formula relating local unemployment rates to general revenue sharing appropriations. The Public Service Employment expansion program provides public or nonprofit jobs to the long-term unemployed whose annual income does not exceed 70 percent of the lower living standard income level and to members of AFDC families. Funds are distributed to "prime sponsors" under a distribution formula intended to concentrate jobs in areas of high unemployment. Under recent amendments, all Title VI jobs above "sustainment" levels must be in "projects" lasting no more than one year, and at least one-third of Title VI funds must support project jobs in nonprofit agencies.

Of particular importance to policy makers is the employment impact of this counterrecessionary package. With the nation's unemployment rate continuing to register well above six percent throughout 1977, the job-creation effects of the ESP are regarded as critical to both easing local unemployment and strengthening a general economic recovery. Indeed, recent increases in employment and reductions in unemployment during the first quarter of 1978 may in part be due to the ESP. Not surprisingly, debate over the policy options of the ESP centered on the number of jobs which would be created by one measure or another, and a variety of job-creation estimates have been published. Moreover, policy makers have recognized that the economic and social burdens of a recession are not distributed evenly among regions, industries, or socioeconomic groups. Securing the economic health of the nation's major cities has become a major policy objective, joining the continuing goal of mitigating employment problems of the poor and unskilled. Thus, there is also considerable concern over the geographic, sectoral and sociodemographic targeting of new jobs.

Given the policy priorities placed upon job creation and lowering unemployment, it is essential that policy makers and researchers understand the dynamics of job creation and the likely employment consequences of the ESP or other possible counterrecessionary measures. Unless one is a trained economist, however, this understanding is not easily gathered from the literature. Confusion is created by a welter of such terms as "direct," "first round," "off-site," "induced," "primary," "indirect," "long-term," and "secondary" job creation. Debates on important issues such as local substitution or displacement, capital crowding, labor supply responses, leakages and multipliers are lost on the layman, who may be unfamiliar with the literature, terminology and theoretical underpinnings of the discussions. The econometric and input-output methods used to model and estimate job creation impacts are difficult to understand, being presented in mathematical or technical economic terms. And in any case, the state of the art in estimating total job creation effects, particularly at sub-national levels, is not as advanced as policy makers ideally would desire.

This paper attempts to summarize and clarify some of the more important literature and concepts underlying job creation debates. It is aimed principally toward readers who are not labor economists, persons who, though

they may be familiar with job creation policies and programs, are not fully conversant with the economic literature and complexities in the dynamic of job creation. First, a brief explanation of the general theory of multipliers provides some theoretical underpinning to discussions on job creation. Second, the tools of the ESP are discussed in terms of the relative magnitude and distribution of their likely gross job creation effects. Third, several key issues in the job creation debates--substitution, capital crowding, spending/saving behavior and local labor market responses--are reviewed with respect to their likely effects upon job creation.

Fourth, in Section 5.0 we attempt to explain in simple terms the principal tools used to estimate new employment--input-output and econometric models--the assumptions they make and the main strengths and weaknesses of each. Several of the more important applications of such models, and their resultant national and subnational estimates are reviewed in Section 6.0. This brief broad-brush state of the art review of current methodologies and estimates is somewhat more technical than earlier sections of the paper, but is discussed in terms which should make it easily accessible to a non-technician.

Finally, initial steps toward a methodology which researchers can use to estimate the rough scale of local (or subnational) employment effects are explored. This methodology is intended to reduce the data collection needs (and costs) associated with complex local model-building; in some respects, it parallels the case-study approach being applied by the Brookings Institution to estimate local substitution and displacement.

2.0 MULTIPLIER THEORY

Economists and policy makers alike know that a dollar of expenditure has a greater economic impact than simply the dollar itself.[1] An economic disturbance (change) in the economy has secondary effects of a certain, at least crudely measurable magnitude. The analogy of a stone thrown into a pond is useful to illustrate that secondary economic effects rise, spread, diminish and eventually die over time. The stone creates a large splash and waves (in all directions) which fade with time, in a manner that is dependent on the size of the initial splash and on the distance from the impact site. Thus, each dollar of new public or private spending will generate a new flow of goods and services whose value is greater than one dollar. This is the so-called "multiplier effect." For convenience in discussion, the splash and waves of the multiplier effect are described in terms of three economic effects: direct, indirect, and induced.

The initial spending in a government program is in the form of the purchase of supplies, materials, and/or services; and the payment of wages, salaries, and fringe benefits. This leads directly to production of some new output, in the form of goods and/or services. These results of the initial expenditure (the splash of the stone in the earlier analogy) are called direct effects: direct new income, direct new employment, and direct new output.

In the production of the new direct output, however, demand for labor and materials is stimulated. The initial producer purchases materials from other suppliers who, in turn, may need to employ more labor and purchase additional materials and labor from other suppliers in order to meet their own expanded output goals. This dynamic results in indirect impacts upon income, employment, and output.

Moreover, the direct and indirect increases in income stimulate additional consumer spending. Workers with more money (or more workers with money) increase their consumption purchases, which increases output and employment still further. These consumption-related effects are called

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[1] This will always be true except for the theoretically interesting but politically irrelevant case of an economy operating at full employment of labor and industrial capacity, with a fixed labor supply and no important trade relations with other economies.

induced impacts upon income, employment, and output. The same phenomenon occurs with businesses. As their profit incomes rise, they will buy more from, and sell more to, one another. With the general increase in purchasing power, some firms (whether or not they were involved in the original stimulus directly) may see an opportunity to increase their own sales, if only they are able to expand their plants or build new ones. Thus, the initial spending increase may ultimately lead to an induced increase in private capital investment activity as well. Such an induced increase in investment spending is sometimes called an "accelerator effect" in the economics literature.

The effects of the initial expenditure are called the direct effects. The indirect and induced effects that are caused by the direct stimulus are called the multiplier effect. The sum of the direct, indirect, and induced effects is the total effect. The "multiplier" itself, a number which is often used to estimate unknown total effects from known direct effects, is simply the total effect divided by the direct effect.

Multipliers will always have a value of greater than one, but they will not be unlimited in scale. Workers, firms, and government agencies, for example, will almost always respend less than the full dollar of new income, choosing to save part of it. Moreover, larger than normal inventories may absorb some or most of the increased demand for materials, muting the indirect and induced effects. Finally, in a particular geographic economy--whether it be a city, state, region, or country--part of each dollar of new income will be used to purchase imports that are produced outside the area. Construction firms in Phoenix, for example, may purchase steel from Pittsburgh or, for that matter, from Japan. These phenomena constitute "leakages" from the flow of new spending, at least in the short run.[1] It is these leakages which cause the ripples of the multiplier to dampen and eventually disappear after continued rounds of spending.

Generally, three types of multipliers are used or cited in the literature. "Output multipliers" measure the direct, indirect, and induced new supplies of the goods and services themselves, generally valued at current market prices. Given estimates of the size of the flows of new income (wages, salaries, interest payments, rents, and profits) that are created

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[1] In the long run, the import leakages may lead investors or "outsiders" to build new productive capacity inside the city, region, or country itself.

for each extra dollar of new goods and services produced, the output multiplier can be transformed into an estimate of the "income multiplier." Similarly, equipped with data on the number of new person-hours (or months, or years) of labor that it takes to produce a dollar's worth of new output, the output multiplier can be converted into an estimated "employment multiplier." Although most macroeconomic analyses, for example, the work of the Council of Economic Advisors, are concerned with income multipliers, for our purpose it is the employment multipliers that are of greatest interest.

The dynamics of the economy which lead to employment multiplier effects--national or local--are of principal concern in this paper. However, it should be stressed at the outset that the size of an employment multiplier may not be indicative of the total job creation effect of the economic stimulus. For example, two different government programs may each create a total of 100,000 new jobs. However, in the first program 70,000 of the jobs are direct, while in the second only 40,000 direct new jobs are created. Thus, the employment multipliers for the two programs would be 1.43 (100,000 ÷ 70,000) and 2.50 (100,000 ÷ 40,000) respectively, even though they each have the same total job creation effects. Larger multipliers do not necessarily mean larger total job creation impacts.

More important than the employment multipliers are the separate types of employment effects of different economic stimuli (in our discussion, government programs). To summarize, these effects may be considered as three distinct job creation impacts:

A. Direct

1. Direct employment -- new jobs created by the expenditure of public (or private) funds in the form of direct wages, salaries, and fringes.

B. Multiplier

2. Indirect employment -- new jobs created in the industries supplying the program with materials, goods, and services, and then in the industries supplying them, etc., etc.

3. Induced employment -- new jobs created by the increased purchase of goods and services by the workers who have been directly and indirectly employed, and by the increased investment undertaken by private firms in response to rising sales expectations.

The distinctions made between direct, indirect, and induced employment are not simply of academic or theoretical interest. They are of critical importance to policy and program evaluation. For example, in most debates about countercyclical employment options, principal attention is focused upon direct jobs. The reasons are various. Direct employment effects are conceptually easy to understand and are much more susceptible of measurement than are indirect or induced effects. Moreover, the magnitude and distribution (among demographic groups, over space) of direct effects can be controlled in the design and implementation of a stimulus program much more than can indirect or induced impacts. Finally, indirect and induced employment is sometimes ignored simply because decision makers are unaware that such impacts occur or don't understand that the impacts may be quite different from direct employment in scale and type. Yet, for capital intensive programs (public works, for example), the indirect employment impact may be far greater than direct employment. To ignore it is to ignore the principal job creation effects of the stimulus. And, to the extent that the full dynamic of job creation is more completely understood, policy makers can design programs whose total employment impact is greater and better targeted upon intended groups, geographic areas or industrial sectors.

3.0 DIRECT, INDIRECT, AND INDUCED EMPLOYMENT UNDER THE ECONOMIC STIMULUS PACKAGE

The fiscal tool selected as the vehicle for economic and employment stimulus will have a major effect on the magnitude, timing, and distribution of employment impact. Tax cuts, for example, have been used by many administrations, for example the Johnson administration in 1964, and are currently being considered by the Carter administration for 1978. In general, the employment impact of a tax cut will be slower and smaller than that of direct expenditures and, depending partly upon the nature of the tax package, will be primarily consumption-induced employment, not direct or indirect employment. The employment impact of federal tax cuts is also difficult to target on particular groups or locales, and, unless the program is a one-time rebate, is harder than other tools to turn on and off in response to countercyclical economic changes. And, because of targeting and timing difficulties, a tax cut may be more likely to stimulate inflation. For these reasons, tax decreases are generally less favored as a policy when the principal objective is short-run, at least partly redistributional, job creation.

Among the variety of available fiscal tools, the three elements of the Economic Stimulus Package (ESP) represent quite different approaches to economic and employment stimulus. Part of the reason is that the programs have different objectives. The principal objective of Public Service Employment (PSE) policy is to increase state, local and nonprofit employment of particular types of workers. Under the Local Public Works program (LPW), the objectives are not only to create jobs but also to provide public "infrastructure," capital assets which should have longer term benefits to the local area. Anti-Recessionary Fiscal Assistance (ARFA) is a more general fiscal stabilization tool for state and local governments.

As a consequence of different objectives and variations in program implementation, the ESP programs can have a variety of social and economic benefits and costs which have little to do with job creation. It is the purpose of this paper, however, to focus upon likely gross employment impacts, without attempting to judge the tradeoffs between employment and other social or economic consequences. Thus, observations on employment impact must not be construed as assessments of a program's capacity to meet its full objectives.

3.1 The Relative Magnitudes of Employment Impacts

Public Service Employment (PSE) under Title VI of CETA invests directly in putting people to work in public or nonprofit jobs. For several reasons, one would expect that its principal employment impact will be direct, with indirect effects being comparatively small. First, prime sponsors are limited to 15 percent for materials and administrative expenses, with no explicit allocation for the purchase of materials or equipment. Policy makers have envisioned PSE as almost a "pure service" program utilizing space, equipment and supervision at the local level that are required only in limited quantities or are already available or obtainable from other sources. With about 85 percent of program funds spent upon wages and fringes, direct employment will be substantial. Second, the magnitude of direct employment impact is enhanced under PSE by program limitations upon wages and salaries. CETA regulations currently limit PSE salaries to a maximum of $10,000 per year, with a target average of $7800. Prime sponsors are not required to adhere to the "prevailing wage" provisions of the Davis-Bacon Act except on construction work which is locally covered by the act. Thus, a $1 billion PSE expenditure, of which 85 percent is spent on labor, will support about 109,000 direct person-years of work at a $7800 average wage, only 94,500 direct person-years at a $9000 average wage.[1]

In contrast, the Local Public Works Program (LPW) is likely to have significant indirect employment impact, but much less direct job impact than PSE. The principal reason is that LPW expenditures will be substantially less labor-intensive than PSE spending. The nature of the construction projects approved for funding requires that a large percentage of the funds be utilized to purchase materials and supplies or to rent equipment. In comparison with PSE, a much lower proportion of direct expenditures is devoted to wages and fringes. No explicit limitation on non-wage expenditures is imposed on LPW projects, and the resultant labor intensity may not be much more than the 22 percent experienced under EDA's Public Works Impact Program (PWIP) during the early 1970s.

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[1] These are not necessarily "new" jobs, since local substitution or other factors may reduce the net job creation impact. See discussion of these points in Section 4 below.

The direct employment impact of LPW will be affected by regulations which require adherence to the Davis-Bacon Act. Under Davis-Bacon, jobs supported entirely or partially with federally-controlled funds must pay the "prevailing wage in the area." This is usually determined by a survey of employers in the area and is most often approximately equivalent to the union negotiated wage.[1] Construction sector wages are relatively high, whether they are union-negotiated or not, often over $20,000 per year on an annualized basis. Thus, for the same total wage bill, the number of person-years of direct employment under LPW will be less than under PSE.[2] For the sake of comparison, a $1 billion LPW expenditure which is 25 percent labor intensive will support only 12,500 direct person-years of work at an average wage of $20,000. If the average wage were $7800 (like the CETA "target average"), over 29,000 direct person-years of work would be funded. The Commerce Department projects that the $6 billion LPW program will create 126,500 jobs, or about 21,000 jobs per $1 billion. The total cost per person-year would thus be more than $47,000.[3]

Whether a project is done under force account or bid account is another factor which may affect the magnitude of direct employment. PSE projects are conducted primarily under force account (for example, persons are hired directly onto the public payroll),[4] while LPW projects are legislatively mandated to be done under bid account. If EDA's evaluation of the

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[1] See Armand J. Thieblot, The Davis-Bacon Act (Philadelphia, 1975).

[2] This does not necessarily mean that the number of persons who receive LPW jobs will be fewer. It is very unusual for construction industry craftsmen and operatives to work full 52-week years, because of the seasonal and cyclical volatility of the industry. Indeed, this is one reason why hourly wage rates are relatively high. One would expect a similar phenomenon under LPW --shorter job duration and, thus, more temporary employees per "full-year slot" than under PSE.

[3] Daily Labor Reporter, Nov. 15, 1977, No. 221, p. A-3. The reader is cautioned to consider all estimates cited here from other sources as illustrative, not definitive. All are based upon projection models which incorporate different assumptions and which have varying strengths and weaknesses. See Sections 5 and 6 below for further discussion.

[4] The term "force account" is not limited solely to work done through direct hires on a government payroll. It also includes subcontracted work, if the governmental unit is the general contractor.

Public Works Impact Program (PWIP) is indicative, bid account projects may provide less employment per dollar of expenditure. Under PWIP, projects done under bid averaged $4559 per person-month, almost double the $2539 per person-month observed for force account projects.[1] Reasons for the marked difference are not clear. Yet, the fact that LPW projects must be let out under bid may result in less direct employment than would occur if they were conducted under force account.

The likely indirect employment impacts of LPW will partly compensate for its relatively small direct employment effects. Jobs are created indirectly as a result of goods and services purchases from the initial stimulus, which increase demand among suppliers (and their suppliers). The high level of materials purchases planned by LPW projects should stimulate considerable indirect employment in the construction materials, manufacturing, heavy equipment, wholesale and retail sectors. In contrast, PSE may stimulate very little indirect employment, since purchases of materials and supplies are extremely limited.

The magnitude of indirect employment effects will generally vary directly with the labor intensity of the supplying firms. Moreover, it will be affected by the current inventory and labor situations of the suppliers. In a recessionary period, inventories of many suppliers may be higher than usual. The inventories may absorb much of the new demand for materials and, unless the firm decides to rebuild inventories to prior levels, will mute indirect job effects. Furthermore, indirect job creation will depend upon whether supplying firms have "hoarded" their skilled labor.[2] In a period of economic downturn, there is a tendency for firms to retain those workers whom they consider most valuable, even at decreased levels of productivity. The less valuable, usually unskilled, labor is laid off. The countercyclical increase in materials demand may merely result in more complete utilization of "hoarded" labor (and capital capacity). Thus, during an economic downturn, indirect employment impact may be lower than in an expansionary period. On the other hand, if the size of the stimulus is substantial or the suppliers

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[1] Anthony J. Sulvetta and Norman Thompson, An Evaluation of the Public Works Impact Program (Economic Development Administration, Washington, D.C., 1975).

[2] See, among others, Walter Oi, "Labor as a Quasi-Fixed Factor," Journal of Political Economy, December 1962, pp. 538-555.

are already fully utilizing hoarded labor, the indirect employment effects could be swift and substantial, since it is relatively easy to hire lesser skilled labor immediately.

Clearly, precise estimates of the magnitude of indirect employment effects depend upon a tracking of the materials supply networks of local jurisdictions or contractors receiving the federal funds. In the absence of such data, the estimates of a recent study may be instructive in establishing broad parameters for the likely indirect impacts of LPW. The Rand Corporation has estimated that, across 22 types of public works projects, a $1 billion expenditure will create about 33,400 direct jobs and approximately 10,725 indirect jobs, an average of .32 indirect jobs per direct job. The indirect-to-direct job ratios, however, vary widely by type of project, from 1.46 for construction of revetments to only .103 for the building of levees.[1] Such estimates are more conservative than Commerce Department projections that the $6 billion LPW program will create 66,000 indirect jobs and 126,500 direct jobs, or .52 jobs for each directly-funded position.[2]

Consumption-induced employment effects result from the new consumer and business spending stimulated by the new wage and sales income. Although the timing of impacts may vary, there is little reason to expect a wide variance in the magnitude of induced effects resulting from different federal stimulus measures. One recent paper makes a reasonable case for the likelihood that the induced employment impact for a PSE program will be comparable to that of a tax cut.[3] A major study of public works projects has concluded that "induced employment does not vary significantly among the types of projects considered."[4] While there is a lack of empirical evidence, the

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[1] See Georges Vernez et al., Regional Cycles and Employment Effects of Public Works Investments (Santa Monica, California, 1977). It should be noted that the Rand estimates of indirect jobs are understated, since they are considering only the jobs created among project suppliers, not the jobs created with the suppliers' suppliers, etc.

[2] Daily Labor Reporter, op. cit.

[3] See Charles Killingsworth and Christopher King. "Tax Cuts and Employment Policy," Job Creation: What Works (Salt Lake City, 1977), pp. 1-33.

[4] Vernez et al., op. cit.

Congressional Budget Office has estimated the total employment effects of a tax cut (which are primarily induced) to be about 30,000-40,000 jobs per $1 billion.[1]

One factor which may affect induced employment should be noted. To the extent that employees or firms save their new income, instead of respending or reinvesting it, induced effects will be reduced or at least delayed. It can be argued, for example, that if the new income is concentrated upon the poor, who cannot afford significant savings and have a higher marginal propensity to consume, it is more likely to be respent than if it is concentrated on other groups. To the extent that PSE is effectively targeted upon poorer population segments, it may induce a somewhat higher level of employment than LPW or a tax cut.

In the preceding discussion, no mention has been made of Anti-Recessionary Fiscal Assistance (ARFA), the countercyclical revenue sharing program under Title II of the Public Works Act of 1976. This represents a third and quite distinct vehicle for economic stimulus. Under ARFA, state and local governments receive funds when the national unemployment rate exceeds six percent. ARFA essentially provides block grants, with very few constraints placed upon local use consistent with the goal of maintaining "levels of public employment and of basic services customarily provided." In effect, ARFA is more of a countercyclical economic stabilization policy than an explicit employment tool.[2]

Employment impact, to the extent that it is traceable at all, will vary greatly according to local conditions and how local governments spend the money. At one extreme, if ARFA is used merely to increase fund balances or to avoid borrowing to finance already committed projects, employment impacts will be negligible. The use of ARFA funds for local tax relief would also have virtually no direct or indirect employment effects, but would have some induced effect through consumer (and perhaps investor) spending out of increased disposable income. Capital expansion or building projects would likely have impacts which somewhat resemble those discussed for LPW. Finally, the expansion of services (or the maintenance of services and jobs which

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[1] Congressional Budget Office, Temporary Measures to Stimulate Employment: An Evaluation of Some Alternatives (Washington, D.C., 1975).

[2] See Edward M. Gramlich, "Evaluating Countercyclical Revenue Sharing," draft, November 1977.

otherwise would have been cut) could have significant direct effects, probably minimal indirect impact, and some induced effect, somewhat along the lines of a PSE program.

It is not possible to predict the relative mixture of uses to which state and local governments will put ARFA funds. However, findings from the Brookings Institution study of general revenue sharing may be suggestive.[1] Accepting all the caveats of applying data and expenditure categories from one program and research focus to a somewhat different program and different analytic purpose, the distribution of general revenue sharing spending may nonetheless provide a very rough idea of how ARFA will be used. In the Brookings sample, about 10 percent of funds (11.8 percent for local government, 8.6 percent for states) was used to increase fund balances or avoid borrowing. Approximately one-fifth of the funds (20.5 percent for local governments, 21 percent for state governments) was used for purposes which would have only consumption-induced employment impact like that associated with a tax cut. About one-quarter of funds (23.9 percent for local governments, 28.3 percent for state governments) were spent to expand or maintain services, a use which could have significant direct employment impact and a general pattern of effect similar to PSE. Fully 41.4 percent of local funds and 21.0 percent of state funds were spent on capital expansion and might be expected to stimulate employment effects similar in type to LPW.

This distribution of funds use is substantially different from that assumed by the Congressional Budget Office in 1975[2] when they projected a substantial employment impact (72,000-100,000 jobs per $1 billion) from a general revenue sharing policy option. For the upper bound estimate, CBO assumed that all funds would create direct jobs at an average of $13,000 per year. The lower estimate assumed 45.5 percent of expenditures for direct employment, 37 percent on tax cuts and 17.5 percent for debt repayment or addition to surplus. If the Brookings distribution for GRS is close to correct, then the CBO estimates of employment impact are too high.[3]

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[1] Richard P. Nathan, Charles F. Adams, Jr., and Associates, Revenue Sharing: The Second Round (Washington, D.C., 1977), especially pp. 29-67. It should be noted that the purposes, and thus the local uses of funds, may be somewhat different for GRS and ARFA.

[2] CBO, op. cit.

[3] The Brookings Institution is currently conducting a case study-based assessment of the Economic Stimulus Package in selected local areas. In the case of this study, a much better idea of the uses of ARFA funds will be gained.

3.2 Distribution of Employment Effects

The impact of a recession does not fall evenly on sociodemographic groups, geographic areas or industrial sectors.[1] Indeed, anti-recessionary programs and their specific provisions are usually designed to mitigate or at least address the perceived imbalances. PSE programs are directed principally at lower income and longer-term unemployed persons as well as at persons belonging to selected population segments. LPW is initially targeted at the construction workforce and industry, as well as its suppliers, while ARFA explicitly recognizes the fiscal problems of geographic areas. Under all three programs, the provision and/or size of funding is in some way linked to national and local unemployment rates.

Sociodemographic Distribution

Under current regulations, the expanded PSE program is more explicitly targeted upon particular sociodemographic groups than is LPW, with ARFA employment effects being essentially untargeted. Program eligibility criteria specify length of unemployment and income, as well as giving certain preferences to selected population segments. In principle, at least, PSE most directly serves those populations most severely affected by a recession, those at the end of the hiring queue whose education, skills, employment experience and available labor market make them the "last hired-first fired."[2] From the point of view of distributional equity, PSE would appear to be a very appropriate policy. Moreover, since most jobs created under PSE are direct, the program regulations target the lion's share of total employment impact to the disadvantaged. This has a further benefit of increasing transfer savings to federal and state government, since disadvantaged populations are likely to be relatively dependent upon AFDC, SSI, General Relief, Food Stamps, Medicaid and other cash or in-kind transfer programs. Induced employment effects of PSE, like induced impacts of any program, are not explicitly targetable to sociodemographic groups.

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[1] See Edward Gramlich, "The Distributional Effects of Higher Unemployment," BPEA, Vol. 2, 1974.

[2] Whether PSE has actually achieved this objective is another question. For example, DOL data show that, in the final quarter of 1976, only about half of Title VI participants were economically disadvantaged and only 25 percent had less than 12 years of education. At that time, however, CBTA regulations--in contrast to current provisions--did not explicitly require hiring of the disadvantaged.

Under LPW, direct employment benefits are aimed primarily at construction craftsmen and operatives. This targeting almost guarantees that women and minority workers will not be proportionately represented. However, LPW may very well serve a lower income constituency. Although not at the end of the hiring queue, construction workers depend upon an industry which is plagued by cyclical and seasonal downturns and is particularly sensitive to recession. Thus, the prime target of LPW direct employment is certainly a population which tends to be greatly affected by an economic downturn. Moreover, state and federal governments will tend to benefit from the reduced unemployment compensation benefits caused by LPW employment, since construction workers are usually eligible for maximum benefits.

In the public eye, however, the direct employment constituencies of LPW are often perceived as far less in need of support than PSE constituencies, a perception which is supported by the fact that 48 percent of employment under PWIP went to the highly skilled (and highly paid). A recent study of EDA's Job Opportunities (Title X) program, however, casts some doubt on this popular conception. Under Title X, direct employees were low income ($6300 annual mean), mainly unemployed (71.1 percent at the time of hire), surprisingly long-term unemployed (28.7 weeks average), and highly transfer-dependent. Indeed, the Title X constituency was more economically disadvantaged and unemployed than CETA PSE participants.[1] Although Title X was more labor-intensive than LPW and offered a somewhat different mix of employment positions, the profile of its employee constituency may be somewhat indicative of the socioeconomic status of LPW employees.[2]

It should be added that the sociodemographic targetability of LPW is limited due to the fact that a substantial proportion of its employment impact is indirect. The indirect employment may reach those in great cyclical need if the supplier industries are largely dependent upon construction activity and heavily affected by construction downturns. However, little is known

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[1] See Robert Jerrett, III et al., Effects of Job Creation (Abt Associates, Inc., Cambridge, Ma., 1977).

[2] Interestingly, the income, employment status, and transfer dependency characteristics of Title X employees were remarkably consistent across project types. Construction project workers did not vary greatly from service project employees or those on "soft" public works projects. Ibid.

about the dynamics of local materials supply, and, in any case, the socio-demographic targeting of the indirect impacts is beyond the control of policy makers.

Some economists maintain that programs such as PSE, and possibly LPW, which target countercyclical jobs to the worst off population segments, can have the effect of "cheating the Phillips curve."[1] On the assumption that the relatively low wage, low skill PSE constituency is on a relatively flat Phillips curve, they argue that the possibly inflationary aspects of the program upon wages will be mitigated. The same phenomenon could occur under LPW, if the work were force account rather than bid account and at a wage less than that determined under Davis-Bacon. If unemployed construction workers were willing to be hired onto the public payroll at a rate less than the private market wage, the incentive to return to private employment would be greater and the possible wage inflation consequences of program-caused labor bottlenecks would be lessened.[2] Under current LPW regulations, this, of course, is not the case.

Geographic Distribution

Countercyclical policy objectives make the geographic targeting capabilities of a stimulus tool particularly attractive. In an economic downturn, different regions or SMSAs are not affected equally. Economic activity in New England or the Midwest may be severely affected, for example, while growing areas of the South and Southeast continue to thrive. Moreover, the important current policy objective of improving the economies of major urban areas, especially older cities, increases interest in stimulus tools whose impacts can be localized.

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[1] See Martin Baily and James Tobin, "Macroeconomic Effects of Selective Public Employment and Wage Subsidies," BPEA, Vol. 2, 1977. The Phillips curve posits an indirect relationship between the tightness of the labor market (measured by the unemployment rate) and the rate of change in wages. The more vertical (or steep) this curve is, the greater will be the rate of change in wages associated with the change in the unemployment rate. It should be noted that presently there is very much controversy in the economics profession on the generalizability of this theoretical construct.

[2] The same principle is behind the current administration's welfare reform proposals, which peg public jobs at a lower wage than the prevailing market wage. The purpose, however, is different. Inflation is less the issue than reduction in public support burdens.

Considering the mechanisms of the ESP, PSE appears to be the most targetable. If residency requirements are strictly enforced, the direct employment impact of PSE will be concentrated in the specific geographic area which received the grant. Since indirect effects are minor, the new wages will stay in the local area and probably be largely respent locally. Thus, one would also expect much of the consumption-induced employment to occur in the local area, especially in service, wholesale and retail sectors. Given current regulations, therefore, PSE appears to be a tool which can be highly targeted to specific areas.

Under LPW, employment impacts will be more geographically dispersed. LPW regulations do not stipulate that employees be from the local area, and, since all work is accomplished through contracting mechanisms, a residency constraint would be hard to monitor. Also, there are likely to be migration effects as unemployed or underemployed skilled workers move to the areas with the jobs. If the experience of PWIP is indicative, where two-thirds of the skilled positions went to nonresidents, the geographic targeting of direct LPW jobs may be limited.

Furthermore, under LPW, the bulk of employment impact will be indirect and induced. The location of indirect jobs will depend upon the location of the suppliers of local contractors. In principle, states or localities could mandate that materials and supplies be purchased locally, or as locally as possible, thereby retaining more indirect employment in the target area. However, such formal or informal constraints are difficult to implement, and it is likely that many indirect jobs will be created outside the local area. Moreover, since new disposable income is dispersed, induced employment will also be relatively untargeted. Indeed, a recent study of public works programs concluded that "... indirect and induced employment effects of public works investments accrue largely in areas other than where the projects are located."[1]

Industrial Targeting

Direct PSE jobs are all in the public or nonprofit sectors, and, because of this, some economists and policy makers have been suspicious of PSE, feeling that somehow only private sector jobs are "real." They maintain that public employment under PSE will do little to assist the nongovernment economy

[1] Vernez, et al., op. cit., p. 161.

of an area. PSE proponents, however, point out that if PSE lowers unemployment and has any positive effect upon welfare burdens, crime, etc., it will improve the "climate" for private investment. Moreover, the consumption-induced employment stimulated by PSE, which may be as great as or greater than employment induced by other fiscal tools, will be in the private sector. Most of the impact will likely be felt in the consumer goods industries and among retail and wholesale establishments.

LPW is explicitly targeted to provide direct jobs in construction. Indirect effects will be stimulated in construction materials, heavy equipment, manufacturing, wholesale and retail sectors. Moreover, the sectoral distribution of induced employment will probably not differ greatly from that of PSE or any other stimulus, there being no empirical evidence that the distribution of induced effects varies greatly for different programs. Thus, though the total employment impact of LPW is likely to be smaller than that of PSE, it will all be concentrated in the private sector where it could stimulate lagging local economies.

Summary

The likely differences in the gross employment impact and targetting of PSE, LPW and ARFA can be illustrated in summary form by presenting a hypothetical example, as in Table 1 on the following page.

Table 1

Hypothetical Employment Effects of $1 Billion Expenditure on Each ESP Program

	PSE[1]	LPW[2]	ARFA[3]
Direct Employment	large (c. 100,000)	smaller (c. 21,500)	smaller (c. 24,000)
Indirect Employment	small (c. 9,000)	larger (c. 43,000)	medium (c. 15,000)
Induced Employment	medium (c. 35,000)	medium (c. 35,000)	smaller (c. 28,000)
Total Gross Employment	c. 144,000 person-years	c.100,000 person-years	c. 67,000 person-years
Sociodemographic Distribution	highly targetable	less targetable	untargeted
Geographic Distribution	highly targetable	less targetable	less targetable
Industrial Distribution	less targeted, public and non-profit	targeted, construction and materials supplies	untargeted

[1]Presumes labor intensity of 85% at an average wage of $8500 per person year.

[2]Presumes 30% labor intensity at an average wage of $14,000 per person year that 2 indirect jobs are created for every direct job.

[3]Presumes 25% used to maintain labor-intensive (85%) services at an average wage of $12,000; 30% spent on capital expansion similar to LPW, 25% to cut taxes or avoid tax increase; 20% to increase fund balances or avoid borrowing.

Presume that $1 billion is spent on each of the three programs, under the program assumptions detailed in the table. (Changes in the assumptions would, of course, change the effects.) Under these circumstances, PSE would have by far the largest direct effect, with ARFA (primarily because of the $250 million devoted to labor-intensive services) and LPW having only one-quarter to one-fifth as much direct impact. In contrast, LPW would have the greatest indirect effect. Induced employment would probably be about the same for PSE and LPW, since there is no strong evidence of different consumption patterns resulting from alternative federal investments. Employment induced by ARFA would be somewhat smaller, since $200 million of the ARFA funds were, in this example, used to maintain or build fund balances and, thus, taken out of the consumption stream. In sum, under this hypothetical scenario, the total gross employment impact would be largest for PSE, smallest for ARFA.

In terms of distribution, PSE offers the greatest opportunities to target the sociodemographic and geographic employment impacts. LPW, however, has a more specific industrial focus and more immediately affects the private sector. ARFA, by the nature of the program, is the least targeted, though local decisions on spending could be made with distributional objectives in mind.

4.0 OTHER FACTORS AFFECTING NET EMPLOYMENT IMPACT

A number of factors--apart from the countercyclical policies chosen and the rules and regulations governing the implementation process--can affect the magnitude and/or distribution of direct, indirect, and induced employment impacts. Economists are currently engaged in major debates over such phenomena as local substitution, capital crowding, government and personal saving behavior, and labor force participation responses to economic stimulus. In each case, economic behavior may reduce or redistribute new job creation impacts, or at least affect the unemployment rate, regardless of the countercyclical tool used. A brief discussion of these issues will help to make policy makers and researchers more aware of the complex dynamic in the economy which results from stimulus measures and of its implications for job creation.

4.1 Local Budget Substitution

In Section 3, the expected general scale of employment impact of each major component of the ESP was outlined, accompanied by several estimates from other studies. This discussion, and the estimates, concerned gross employment impact, the numbers of jobs which would be created if there were no intervening effects. However, there may be a great deal of difference between gross and net employment effects, the latter representing the real job creation impact of a program, after all administrative and economic consequences of the program have been taken into account. In almost every instance, net impact will be less than gross impact, although there are hypothetical situations in which it will be greater.

One of the principal factors affecting job creation is local budget substitution (sometimes called displacement). Substitution occurs when a state or local government uses federal money in place of its own funds for a purpose which would have been accomplished even in the absence of the federal aid. In other words, the federal funds are "substituted" for the local funds, instead of supplementing local funds and creating "new" economic activity. In the case of PSE, substitution boils down to a question of the number of employees who represent a direct net addition to the payroll, over and above the level of employment which would have been attained without PSE. For public works programs, the issue is whether the state or local government would have financed the project(s) through some other mechanism if the federal government hadn't

funded them. In considering anti-recessionary revenue sharing, substitution is less of a policy issue, since the program is intended to allow governments to maintain their present employment or expenditure levels or to continue the secular or short-term trend of increase. However, when estimating the job creation effects of ARFA, the degree to which substitution occurs is directly relevant.

The debate on substitution has been particularly vigorous in reference to PSE.[1] Indeed, opponents of PSE as an instrument of employment policy often cite substitution as the most important negative consequence of the program. For example, a Ford administration official said in a Congressional hearing that "we have found. . . after three years only one or two net new jobs remain out of ten supposedly created originally."[2]

Initial estimates of the magnitude of substitution effects have suggested that substitution in employment programs does substantially affect net impact. For example, Johnson and Tomola estimated fiscal substitution for PEP and early PSE to be 60 percent after one year, 67 percent in the long run (two years).[3] The authors have recently revised their assessments, after further data analysis and reestimations, concluding that "the fiscal substitution effect of PSE is very small for one or two quarters..., but then rises to about 100 percent after five quarters."[4] The National Planning

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[1] Initially, the CETA PSE regulations explicitly stated that a local or state government could not, under any circumstances, substitute the federal money to retain existing employees or use it to fund previously planned hires. As the local and state fiscal situation worsened with the recession these strict regulations were eased up a bit. Subsequently, one of the main arguments for the appropriation of countercyclical revenue sharing funds was that they would complement the PSE funds and insure that PSE money was spent for direct hires.

[2] Quoted in Charles Killingsworth, "The Role of Public Service Employment," Proceedings, IRRA Spring Meeting, Tucson, Arizona, 1977, p. 490.

[3] George Johnson and James Tomola, "The Efficacy of Public Service Employment Programs," Technical Analysis Paper 17A, U.S. Department of Labor, 1975.

[4] George Johnson and James Tomola, "The Fiscal Substitution Effect of Alternative Approaches to Public Service Employment Policy." Journal of Human Resources, Vol. 12, No. 1, Winter 1977, p.23.

Association, examining the Public Employment Program (PEP), found displacement to be 46 percent in one year.[1] And Fechter, after his own study and a review of the other work, concluded that "... in the long run 60 to 90 percent of the public employment program funds would merely displace state and local funds."[2]

Such estimates have focused attention on the substitution issue and occasioned serious examination of the assumptions and estimation models. Wiseman's careful review of the Johnson-Tomola and NPA estimates led him to observe that they seemed seriously flawed and that slight changes in the assumptions of each would greatly alter the results. He concluded that "I can find no definitive estimates of the displacement effect. The estimates derived by Johnson and Tomola... appear on balance to be biased toward exaggeration of displacement."[3] Borus and Hamermesh have recently come to similar conclusions. A detailed examination of the most recent Johnson and Tomola work showed that small changes in the econometric methodology produced substitution estimates after six quarters ranging from 56 percent to 242 percent. They judged that "based on the econometric studies, we can say little other than there are some jobs created by PSE."[4] Finally, the Brookings Institution, relying on a case-study methodology, has discovered an average short-run rate of only 20 percent employment displacement (only 15 percent fiscal substitution) in the current PSE program at 37 sample jurisdictions.[5]

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[1] National Planning Association, An Evaluation of the Economic Impact Project of the Public Employment Program, Vol. 3, Appendix M (May, 1974).

[2] Alan Fechter, Public Employment Programs (American Enterprise Institute for Public Policy Research, 1975); Alan Fechter , "Public Employment Programs: An Evaluative Study", Paper 19, Studies in Public Welfare (December 30, 1974) pp. 93-123.

[3] Michael Wiseman, "Public Employment as Fiscal Policy," BPEA 1 (1976) p. 83

[4] Michael Borus and Daniel Hamermesh, "Study of the Net Employment Effects of Public Service Employment-Econometric Analyses," Job Creation Through Public Service Employment, an Interim Report to the Congress of the National Commission for Manpower Policy, Vol. III (March, 1978) pp. 89-149.

[5] Richard P. Nathan et.al., "Monitoring the Public Service Employment Program -- Preliminary Report," Job Creation Through Public Service Employment Vol. II.

The earliest PSE studies implied that, to the extent that there was substitution, the economic and employment impacts of substituted funds were negligible. More recent literature, however, recognizes that the local funds "saved" through substitution are put to some use, a use which may itself have employment impact.[1] Thus, in order to estimate the net effects of substitution upon new income and employment, it is necessary to know how the displaced local funds are used. The new employment effect of a federal program will be the jobs created by the unsubstituted federal funds plus the jobs created by the local funds freed up by substitution.

For example, if the local funds were used to cut taxes (or to avoid a tax increase) employment effects would primarily be induced, like those of a tax cut. If substitution essentially built local surpluses or avoided debt, there would be virtually no employment impact.[2] New capital investments would have indirect (and induced) effects, while new services would have direct (possibly indirect) and induced employment impacts. Indeed, it is possible, though not likely, that substitution could increase net employment, if the displaced local funds were spent for much more labor-intensive purposes than the federal funds were intended to support. As a corollary to this, it is likely that substitution will reduce net employment most when the federal investment is intended to be very labor-intensive (for example, PSE).[3]

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[1] See, for example, Killingsworth and King, op. cit. and Borus and Hamermesh, op.cit. Recent CBO estimates also assume that local funds freed up by substitution will have impacts, the effects being similar to those of general revenue sharing funds.

[2] One study found that the leakages of federal funds into budget surpluses were substantial. However, this study was done prior to the recent recession, when it was much more likely that federal grants would end up as budget surpluses. See Edward Gramlich and Harvey Galper, "State and Local Fiscal Behavior and Federal Grant Policy," BPEA 1 (1973), pp. 15-58.

[3] It should be noted that in the case of PSE, budget substitution might have an unplanned positive effect if it frees up local funds to purchase materials or equipment necessary to support the jobs subsidized under PSE.

The extent of substitution is likely to vary with other factors. To quote Fechter:

> There is evidence that it (displacement) is smaller in the short run than the long run, making PSE more feasible as a short run job creation program. A priori one might expect a smaller amount of fiscal substitution when PSE funds are unanticipated by the local administrators, when the program is small in scale, when the skill mix of PSE participants does not match the skill mix of local public employees, when funds are spent on new activities, when the local government is facing fiscal hardships, during recessions, or when the program is administered on a project (rather than a revenue sharing) basis.[1]

The effects of substitution on the timing and targeting of job creation may be as important as its effects on the magnitude of new employment. In most cases, substitution will result in lags in the stimulus effect of displaced funds, perhaps subverting the countercyclical objectives of the program. Moreover, the larger the substitution effect, the less control there is over geographic, sociodemographic or industrial targeting. The uses to which displaced local funds are put may result in distributional effects which are inconsistent with the goals of the federal program.

In sum, it is fair to conclude that many dimensions of the substitution debate are as yet unresolved. Empirical evidence is incomplete,[2] and the econometric estimates which have been made depend heavily upon inferences and assumptions. One must concur with Killingsworth that "no one has (yet) discovered a reasonably defensible estimating technique."[3]

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[1] Alan Fechter, "Job Creation Through Public Service Employment Programs," in Robert Taggart, ed., op. cit., pp. 127-128.

[2] The study of PSE by the Brookings Institution is yielding substantial empirical data, but it includes only 42 jurisdictions and does not yet encompass longer term effects.

[3] Killingsworth and King, op.cit., p. 24.

4.2 Federal Financing--The Capital Crowding Issue

The cornerstone of most countercyclical policy options is direct new spending, financed by federal government debt. As the government spends more money than it receives in taxes, the national debt is increased. To do this, the federal government enters the capital market by floating bond issues at certain interest rates. This financing approach essentially relies upon the faith of the American people (and businesses) and that of other countries in our future production capacity. That is, the economy will eventually be operating at a high enough level of activity and productivity to generate taxes which lead to a balanced budget or a generally acceptable level of continuing debt.

Some argue that the use of deficit federal financing may eventually reduce, or even cancel out, the economic and employment impacts of a countercyclical program. Monetarist thinkers such as Milton Friedman maintain that government borrowing through the capital markets will "crowd out" private borrowing, unless accompanied by permissive monetary policies. What allegedly happens is that government borrowing drives up the cost of capital (that is, interest rates) which in turn causes a decline in private investment spending. As a result, jobs, income and output are not generated somewhere else in the economy. Moreover, consumers may respond to bond-financed public spending by adjusting their consumption downward. It is hypothesized that consumers will save more in expectation of future inflation or taxes. According to one leading researcher, it is unlikely that any fiscal policy will have much influence in the longer run (after two or three years). "An increase in (for example, public employment) jobs of, say, 100,000 will simply reduce private sector employment by approximately that number after adjustment."[1] Thus, the net employment effect will be zero.

Proponents of federal borrowing maintain that the expanded economic activity will provide a stimulus to private investment by injecting new income into the economy. While the capital crowding phenomenon might be

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[1] George Johnson, "Evaluating the Macroeconomic Effects of Public Employment Programs," in Evaluating the Labor Market Effects of Social Programs, Orley Ashenfelter and James Blum, eds. (Princeton University Press, Princeton, 1977). See also the "new macroeconomics" text by Rudiger Dornbusch and Stanley Fisher, MIT Department of Economics, 1977 MS.

serious in a full-employment economy, it is unlikely in an economy with substantially underemployed capital and labor resources.

The discussion of capital crowding effects to date has been largely theoretical, and there are no solid empirical estimates to support the case one way or another. However, in considering the possible capital crowding effects of a particular countercyclical stimulus, a number of variables require attention. First, the scale of the federal borrowing per period of time will matter. It is plausible that federal borrowing of several billion dollars represents hardly a drop in the total money market bucket, which easily exceeds a trillion dollars. Second, the degree of segmentation of capital markets is not really known. One type of financial instrument may not in fact compete with others, and thus the demand and supply in one segment (for example, short-term securities) may have little to do with the demand and supply in another segment (for example, long-term bonds). A case could be made that the federal government competes only with the huge oligopolistic conglomerates whose large long-range investment strategies are unlikely to be greatly affected by short-term fluctuations in interest rates caused by countercyclical government borrowing. Third, even if government is in competition with private investment, the sensitivity of private capital demand to interest rates (that is, the price elasticity of demand) is not fully known. Because of all these information gaps, the depressing impact of federal borrowing on private investment remains hypothetical.

4.3 Government and Individual Savings

The spending and saving behaviors of governments, individuals and firms will affect the magnitude of induced employment impact, but have little effect on direct and indirect job creation. If individuals spend most of their new income and save very little, consumption-induced impacts will be greater. Similarly, if firms invest the new income from a stimulation in economic activity, investment-induced employment will be greater.

With regard to individual spending behavior, a good case is made that induced employment will be greater if new income is concentrated upon the poorest population segments. Generally, lower income individuals and families have a higher than average marginal propensity to consume, new disposable income being spent for basic necessities and support. Thus, a program which provides new net income to the poor is likely to stimulate somewhat higher

induced employment than a program whose direct and indirect employment and income impact is more evenly distributed among income groups. In addition, since the money will be respent quickly, induced employment effects will quickly be felt in the economy.

A program focusing direct employment upon low income groups, however, will not necessarily concentrate net new income on such groups. Employment may mean the loss of unemployment compensation or the loss or reduction in transfer payments such as cash welfare and Food Stamps. Moreover, wages will be taxed (though presumably at a low rate) and FICA is likely to be withheld. The result may only be a small increase in net income among the poor. The same dynamic will occur among higher income groups, with tax rates being higher but transfer losses being lower. For this reason, the continuing spending behavior of federal and state governments will affect induced employment.

It is well known that in funding social programs, particularly countercyclical employment programs, governments get back part of what they initially spend. The program appropriation, or gross government cost, is reduced by returns to government in the form of increased income, sales and excise taxes; increased employee FICA payments and employer contributions to FICA and unemployment insurance funds. Furthermore, governments save additional money when the program reduces unemployment compensation, cash transfers and in-kind transfer payments which would have been made in the absence of the program. The gross appropriation minus returns and savings to government is called the net cost of a program.

Since part of the initial program stimulus is returned to governments, induced effects will be influenced by what governments do with the returns. If returns are respent, induced employment will be greater, though the effects could be somewhat delayed. If the returns are not respent, induced employment will be reduced as the new direct and indirect income "leaks" from the private sector back to the treasury. In general, during an economic downturn or a recovery phase, the federal government--which receives a majority of the returns--has in recent years continued its deficit spending. Thus, federal government saving behavior, at least, is unlikely to greatly reduce ultimate induced effects of the initial stimulus. In any case, the greater the returns are to governments, the more vulnerable induced impacts are to government saving.

The only rigorous empirical study of net cost effects is a recent evaluation of EDA's Job Opportunities (Title X) Program.[1] Under Title X, first round returns and savings to government are fully 20.4 percent of gross government expenditures. If governments respend none of the returns, total new income and total new jobs are reduced by 10.2 percent, the reduction reflecting the impact of government non-spending upon induced effects. The Title X study also shows that initial expenditures upon wages and salaries return more to governments (23.4 percent of initial spending) than spending upon materials and supplies (13 percent). This will generally be the case and suggests that induced impacts of labor-intensive initial stimuli will be more contingent upon government savings behavior than will the impacts of capital-intensive expenditures.

4.4 Labor Supply Response

The creation of jobs in a local area may cause changes in the local labor force or in labor force behavior. Such changes could compromise the targetability of a stimulus program, but they are unlikely to greatly affect the overall magnitude of job creation impact.

It is possible, for example, that a job creating program will attract new persons into the labor force, persons who perceive their chances of finding suitable work to be improved. The magnitude of this labor force response will be a function of the wages paid, the types of skills required and the composition of the population potentially ready to enter the labor force. In a local area which is experiencing high unemployment and economic difficulties, the response to a significant job creation stimulus might be considerable. Reservation wages of potential workers (i.e., the wage levels at which they are willing to accept employment) may be lower, the pressure on families to have more than one wage earner may be greater, and the pool of "discouraged workers" who have recently left the labor force may be larger.

There is little empirical evidence of the magnitude of this "added workers" response to job creation programs. Often-cited estimates that two new persons will enter the labor force for every ten new public sector jobs

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[1] Jerrett et al., op. cit.

or that ten new jobs stimulated by general growth will add four new workers to the labor force are arbitrary.[1]

The "added workers" response will not reduce direct or indirect job creation. It might slightly change the intended mix of direct new employment to the extent that new labor force entrants are successful in competing for the jobs and are characteristically different from the current unemployed. The major effect, however, will be on the unemployment rate, not job creation. The unemployment rate is defined as $\frac{\text{unemployed in labor force}}{\text{total labor force}}$. Take, for example, a locality with a labor force of 100,000 and an unemployment rate of seven percent (i.e., 7000 unemployed). If 500 new jobs are added to the local economy, the new hiring will reduce the numerator to 6500 and, presuming no change in labor force size, lower the unemployment rate to 6.5 percent. However, if, in response to the new employment opportunities, there are 500 net new entrants into the labor force, the number of unemployed will remain unchanged and the unemployment rate ($\frac{7,000}{100,500}$ or 6.97 percent) will be virtually unaffected. Behavior of this sort is significant, because most program funds are allocated according to formulae based upon unemployment rates. Moreover, changes in the unemployment rate are often regarded as measures of the impact of job creation programs.

A second possible labor force response is that already-employed workers will be attracted to and hired for the newly created jobs. This is particularly likely if the countercyclical jobs offer relatively attractive wages and are not explicitly limited to the previously unemployed. A variation of the same phenomenon can occur if those hired to the new jobs are unemployed but could have expected to be employed for part of the time in absence of the job creation program. In essence, it can be argued that either response reduces net new employment, since some or all of the new employment would have occurred anyway.

Such an argument is based upon the premise that the new jobs increase the competition for the labor supply among already established public and private employers. If the new jobs compete for scarce resources (usually highly

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[1] Michael Wiseman makes this observation in Job Creation: What Works, p. 146. Such estimates are incorporated into the CBO's study (op. cit.) and accepted by both Johnson and Tomola and Alan Fechter, "Job Creation Through Public Service Employment Programs," Job Creation: What Works, pp. 123-144.

skilled workers), for example, labor force "bottlenecks" could be created which might drive up the price of labor (i.e., the wage rate). Similarly, even a program creating low-skilled, low-wage jobs could have the same effect if it were large enough to saturate the available and potentially available low-skilled labor supply. In either case, wages might rise, and eventually local employers (particularly those "at the margin") might either increase their capital/labor mix or even be forced out of business. In the long run, the former effect might enhance economic conditions by raising the productivity of labor. In the short run, however, the result is to reduce net job creation.

Such effects, however, are relatively unlikely in a local area experiencing high levels of unemployment and idle resources. If some of the new jobs are taken by the previously employed, unemployed workers will be hired to fill the vacated positions. Similarly, the "expected employment" of the unemployed who take the countercyclically created jobs will probably be absorbed by other unemployed labor force participants.

A third possible labor supply response is labor migration. An increase in the supply of job opportunities in a local area may attract workers from other areas, increasing the local labor force and the competition for employment. Such migration effects, for example, are not uncommon in the skilled construction occupations. Migration will have impacts similar to those of the "added workers" response. Net job creation will not be affected, but the new jobs may not be reflected in the local unemployment rate. Moreover, a migration response could compromise the sociodemographic and/or geographic targeting objectives of the program. This is particularly likely if the principal target is unemployed residents of a center city, who could find themselves in competition with suburban residents for the new positions.[1] Residency requirements, if enforced, will, minimize the undesired result.

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[1] For evidence of the prevalence of this problem, see Bennett Harrison, Urban Economic Development, (Washington, D.C., The Urban Institute, 1974), Chapter 3.

5.0 METHODOLOGIES FOR ESTIMATING EMPLOYMENT EFFECTS

In Sections 3 and 4, several estimates of likely employment impact or substitution effect have been mentioned. Such estimates have been generated by researchers and policy makers with simulation models, using either empirical or hypothetical data. There are two basic types of tools currently available for estimating job creation effects: econometric models and input-output models.[1] Given the current state of the art, each has serious shortcomings for estimating the scale and distribution of short-run employment effects, particularly at subnational levels. However, an understanding of how the basic tools work and of their major strengths and weaknesses will assist the policy maker and evaluator in assessing their outputs.

5.1 Econometric Models

Econometric models, regardless of their form, follow the tradition of John Maynard Keynes in focusing on the determinants of total final demand (Gross National Product). GNP is the sum of consumption, investment, government spending, and net export demand. The models are a series of equations or mathematical statements describing (presumably) causal

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[1] A third method, the base study approach, has also been in common use over the last 20 years by those concerned with local or regional economic development. It operates by separating an economy into so-called "basic" and "nonbasic" industries and then calculating ratios of jobs in the export (basic) sector to employment in the local ("service," or nonbasic) sector to obtain an "indirect job multiplier." Although this technique is useful, it has been rightly criticized for its simplistic theoretical foundation. It is not considered in our analysis. For a discussion and application of the concept see Raymond Milkman et al., Alleviating Economic Distress: Evaluating a Federal Effort (Lexington Books, 1972), especially Appendix C. For a more detailed critique of this technique see John Jackson, et al., "Urban and Regional Development: A Critical Review of the Literature," (Harvard-MIT Joint Center for Urban Studies, Cambridge, Mass., 1976), drafted for the Economic Development Administration, especially pp. 24-27.

A fourth method, relying upon in-depth longitudinal case studies within a uniform analytic framework, is also coming into increasing use. The Brookings Institution, relying on a network of political scientists and economists, has used this approach in its studies of general revenue sharing and community block grants. It is currently developing the method further in its study of public service employment. See Nathan et. al., op. cit.

relationships among economic variables, which together are able to predict changes in the levels of GNP and its components. The equations describe the way in which a set of endogenous variables determine each other's values, taking into account exogenous (or outside) variables which are fixed and assumed to be unaffected by the endogenous system.

Thus, a typical national model might assume that such factors as government tax rates, foreign demand for American products, and even private investments are exogenous and fixed. Statistical methods would then be used to estimate the parameters of equations designed to explain the relationships between such endogenous variables as prices, value added in different sectors, changes in the level of GNP, income, employment, etc. Depending upon the model, the equations could operate simultaneously (simultaneous model), in sequence (recursive model) or in a sequence of simultaneous equations (block recursive model).

In a simulation of the model, given levels for all exogenous variables are specified, especially those (like government spending) that constitute "policy instruments." These values are plugged into the model and the equation system is "solved," producing different values for each endogenous variable. A variety of statistical tests are applied to judge the strengths or weaknesses of the model's "forecasts." Then, by successfully changing one or more of the exogenous variables and solving the model again, changes in the value of the endogenous variables can be read as indications of the impacts of various policies upon income, prices, employment, etc.

Among the principal national models in use today are the Wharton Economic Forecasting (WEFA) model, the Data Resources Inc. Macroeconomic model (DRI), the Chase Econometrics model and the MIT-Penn Model. These are the most general "Keynesian" models; many more special purpose models are now in widespread use.

5.2 Input-Output Models

Input-output analysis is the second major method available to estimate total employment impacts. Input-ouput models are based on the theories of Wassily Leontiev and others, which focus on the process by which interactions among producers generate both "intermediate" (interindustry) and "final" output of goods and services. The latter constitutes Keynes'

GNP. Thus, in contrast to econometric models, input-ouput analysis focuses upon the production system--the interindustry structure itself--rather than on the components of final demand as such. The Bureau of Labor Statistics houses the largest and most widely used national input-ouput model.

The core of an input-ouput model is its interindustry transactions matrix, whose elements are the coefficients from a system of strictly simultaneous equations which explain how all industries buy and sell from and to one another. Each column represents the production function of an industrial sector vis a vis all other industrial sectors. In so-called "closed" models labor appears as an additional "industry" row in the matrix (or it might be expanded into an industry/occupation matrix mirroring the transactions structure). Final demand (GNP) is represented by a matrix of its own, showing the industrial composition of consumer purchases, investor spending, government spending, etc.

To simulate the impacts of a specified policy, the increase in final demand (whether Consumption, Investment, Government, Exports, or some combination thereof) is entered into the model, distributed among industrial sectors as per the program budgets that underlie the policy. Thus, for example, a PSE program might enter the matrix almost entirely as increased governmental demand for labor. On the other hand, a public works program would enter as increased government demand distributed among construction, wholesale, manufacturing, and retail sectors. On the assumption of fixed coefficient production functions, the simultaneous equations are solved, yielding the amount of new output, income, or new demand for labor which is stimulated by the policy.

5.3 Tradeoffs Between Time Frame and Detail

Econometric models are generally set up to measure quarterly changes in economic activity over a period of 10-20 years or longer. In other words, they are dynamic or "time series" models. However, there is a tradeoff between time frame and the level of detail. It is not possible at the present time in the United States to assemble detailed industry output or employment data for such a long period of time, let alone for time intervals of less than a year. Thus, the time series models tend to be defined at a low level of detail (high level of aggregation). Some only identify 20-30 industrial sectors. Such limited detail may obscure important effects of a countercyclical employment policy.

In contrast, input-output models are usually highly disaggregated, including as many as 450 specific industries and categories of "final demanders" (levels of government, household consumers, investors). Without exception, however, these models are "cross sectional" or static, and they therefore imply that neither technology nor the interindustry composition of final demand changes over time. There are no existing operational tables more recent than 1970. Thus, the price of detail is a static model often based on data which are outdated.

The multipliers which emerge from time series and cross-sectional models will be consistent with one another as measures of the long-run effects of an initial increase in spending, as long as technological, political and other labor force or economic conditions extant when the input-output model was estimated do not change significantly over that "long run, i.e., over the forecast period. However, many economists and political scientists would argue that the conditions of 1963-1970, which form the basis for existing input-output tables, are sufficiently different from the present to call into question the validity of I-O forecasts. Moreover, the cross-sectional model is not capable of estimating short-run multiplier effects or of identifying the time path of the job creation process. For this, one must use a quarterly econometric model, even at the sacrifice of industry detail.

5.4 Average versus Marginal Productivity

We have already referred several times to a major problem with input-output tables: their reliance upon fixed coefficient production functions.[1] Fixed labor coefficients imply a constant rate of utilization of labor by each industry, regardless of the scale of that industry's operations. This is equivalent to assuming a constant rate of average labor productivity. And that, in turn, means that average and marginal productivity are the same, regardless of the level of economic activity. The assumption is simply

[1] Ann Carter and others are currently experimenting with input-output models containing "accelerators," or endogenous investment behavior. Work is also underway on the use of technical coefficients that change over time.

not valid under most economic conditions. Input-output specialists argue that the assumption is "tolerable," and well worth the computational convenience it affords.

Econometric models can avoid this weakness, since their dynamic nature allows for changes in marginal productivity to be incorporated. Nonetheless, the employment estimates of many econometric models are often in fact based upon an average labor productivity. Just as input-output models typically estimate new output and then convert it to new demand for labor, econometric models usually estimate new income created and then independently convert it to employment. The employment effects are often inferred by applying independently estimated averages of income per job (or person-hour, month, or year) to the estimated or simulated income multiplier. Thus, though the model may accommodate marginal changes in productivity, its employment estimates may in fact be the product of an aggregate estimate of average labor productivity.

5.5 Treatment of Household Consumption and Investment Spending

All econometric models are "closed" with respect to consumption. That is, they explicitly include equations that allow consumption expenditures by households to vary as the wage (and other) incomes of those households change. Most contemporary econometric models also contain some form of investment equation(s). It should be noted, however, that the investment equations in large-scale econometric models are--by general consensus--probably the weakest links in these models, and it is not precisely clear how much their inclusion significantly increases the usefulness or accuracy of the multiplier analyses.

In principle, any input-output model can be "closed" with respect to consumption. However, some of the most widely-used examples of this genre, such as the Bureau of Labor Statistics National I-O Table, are often run (e.g., by BLS itself) with the household sector fixed (i.e., not allowed to vary its spending). This is usually a matter of users' budgets, since it is relatively expensive to incorporate the household sector (and labor) as a specific "industry" within the model on a computer. No operational input-output models at present allow private investment to vary with the initially-stimulated changes in output and income.

To the extent that household consumption and/or investment spending are excluded or inadequately incorporated, total employment estimates will be understated, regardless of the type of model used.

5.6 Geographic Level of Detail

One of the most serious shortcomings of current estimating tools is their unsuitability for simulating anything other than national effects. This is of particular concern to employment and economic development policy makers for whom the subnational distributional effects of the new policy and the targeting of instruments upon distressed populations are of considerable importance. It also leaves evaluators without reliable means of projecting the total employment or income impact of a policy upon a given city, state, or region.

All of the large-scale econometric models are aggregate descriptions of the United States national economy. When applied to specific regions or states, the heroic assumption must be made that the local economy acts like the national economy. These subnational models are all "driven" primarily by changes in national variables like GNP. This means that an underlying implicit assumption of interregional balance or "equilibrium" is at work. Several state models have recently been estimated (e.g., for Massachusetts, Pennsylvania, and New York) and, within certain limits, could be used to simulate policy impacts upon these areas. The only fully operational and tested city-level econometric model is for Philadelphia[1]; the development of a similar model for New York City is presently underway.

The National Input-Output Table for 1970 was constructed by the BLS, and it forms the basis for most I-O related empirical research conducted in this country. Several models for individual states and metropolitan areas have been developed by university and consulting institutes, notably a 450 by 450 table for the Philadelphia area. For the most part, small-area I-O tables are of unproven reliability. Moreover, precisely because of their greater level of detail, their technical structures tend to be more unstable over time than the structure of the national table. For example,

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[1] Norman Glickman, "An Econometric Forecasting Model for the Philadelphia Region," Journal of Regional Science (April, 1971).

the appearance or disappearance of one large firm or group of firms in a local area is likely to destablize pre-existing interindustry relationships in that area relatively more than in the country as a whole.

These potential disadvantages of small-area I-O models may be more than offset by the advantages of having a system of sub-federal area tables which are linked to one another via the measurement of commodity trading patterns among the areas. Such a system allows policy makers to evaluate, at least crudely, the impact of new spending in one region (e.g., federal purchases from firms in one region) on income, employment and output in that region, in some other region(s), or in the system of regions as a whole, i.e., the entire country. There are only two operational multi-regional I-O systems currently in use. One is the Multiregional Input-Output Project (MRIO) at MIT, directed by Karen Polenske. The MRIO contains linked tables for all 50 states, but, because it is based on 1963 patterns of inter-industry trade, it is becoming seriously dated. The second is the Regional Industrial Multiplier System (RIMS) developed by the Bureau of Economic Analysis. Under RIMS, the United States is divided into 173 functional economic areas; direct, indirect and induced impacts, in each are estimated differently, using an input-output model and a modified economic base approach and relying upon 1967, 1972, and 1973 data.

6.0 EMPIRICAL ESTIMATES OF NATIONAL AND SUBNATIONAL MULTIPLIERS

In conducting a review of more than 20 empirical studies of national and subnational (regional, state, metropolitan, city and even neighborhood level) multipliers, one is confronted with a literature which is, at best, confusing. To varying degrees, such studies suffer from ambiguities, unexplained inconsistencies, unstated assumptions, the absence of clear or presence of differing definitions and incomplete documentation of methodologies. And, since a detailed and accurate assessment of methodology is beyond the technical capabilities of most, it is hardly surprising that estimates vary widely and that supporters of most policies can find "authoritative" estimates to back their positions. A detailed critique of various studies is badly needed, but such a critique is beyond the scope and purpose of this paper. Instead, we present the main employment impact findings of some of the more plausible and relevant studies.

6.1 National Estimates

Three empirical efforts and one non-empirical study are of particular relevance. Johnson and Tomola's 1975 study of early PSE,[1] the Rand Corporation's assessment of public works programs,[2] and Abt Associates' evaluation of EDA's Job Opportunities Program[3] develop independent empirical estimates. In addition, the Congressional Budget Office has averaged parameter values across a number of econometric models to develop crude summary measures of the GNP and employment impacts of alternative stimulative economic policies.[4]

The Johnson and Tomola study assumes that a federal PSE program creates state and local jobs at a planned average wage and that a negligible amount of the federal grant goes to purchase non-labor inputs. Under 1974 conditions, a $1 billion PSE expenditure is, thus, assumed to create

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[1] Johnson and Tomola, op. cit.

[2] Georges Vernez, et al., op. cit.

[3] Robert Jerrett, III, et al., op. cit.

[4] CBO, op. cit. See also Congressional Budget Office, The CBO Multipliers Project (Washington, D.C., August 1977).

111,100 direct person-years of government employment (at an average annual wage of $9000). Using a national quarterly econometric model of the determinants of both government and total employment,[1] they estimated that, by the end of two years, total national employment impact would be 161,900 person years. The ratio of total to direct jobs (i.e., the employment multiplier) is thus 1.46. Indirect and induced employment are not disaggregated, but all 50,800 non-direct person-years are created in the private sector. Assuming "serious" budget substitution, the authors conclude that--under such circumstances--the $1 billion PSE expenditure creates only 78,900 direct jobs and has a total employment effect of only 87,500 person-years. That is, with substitution, the employment multiplier is only 1.11. Gross government costs per total jobs are only about $6175, but rise to $11,425 assuming substitution.[2]

The estimate of the total effect is based upon a single macroeconomic equation; it is not built up from estimates of the direct, indirect, and induced impacts. Moreover, Johnson and Tomola measure the employment-government spending relationship directly; they do not infer it from independently measured income (or output) multipliers and income (or output) to employment ratios. In these two respects, their work is the most sharply focused and precise (and, as a result, the most narrowly specialized) of all the studies reviewed.

The Rand study of public works combines detailed EDA program data on 22 categories of projects with the coefficients from a national input-output table and the parameters of a national econometric model. Thus, it mixes modes of analysis in developing the estimates. The methodology requires running simulations with a large macroeconometric model for each type of project whose impact is to be studied. Given the computational expense, Rand explicitly works out only three detailed examples sewer plant construction, federal office building construction, and flood

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[1] Ta-Chung Liu and Erh-Cheng Hwa, "A Monthly Econometric Model of the U.S. Economy," International Economic Review, June 1974, 328-365.

[2] See above in Section 4.1 for a fuller discussion of substitution, including Johnson & Tomala's more recent study and the initial empirical estimates by the Brookings Institute.

protection. Their estimates of the job creation effects of a $1 billion expenditure are summarized in Table 2 below.

Table 2

Job Creation Effects per $1 Billion Expenditure			
Job Creation (person-years)	Sewer Plant Construction	Federal Office Building Construction	Local Flood Protection
Direct Jobs	29,404	33,280	45,396
Indirect Jobs[1]	10,478	10,161	9,806
Induced Jobs[2]	69,618	87,559	127,798
Total Jobs	109,500	131,000	183,000
Implicit Employment Multipliers	3.73	3.94	4.05

For the three different project types, $1 billion directly creates only 29,404-45,396 jobs at a gross government cost of about $22,000-$34,000 per person-year. However, Rand estimates of total employment range from 109,500 to as high as 183,000, reducing gross cost per job to only $5465-$9130. Implicit employment multipliers are 3.73 to 4.05. The Rand estimates illustrate that a capital-intensive policy may create a substantial number of total jobs, even if its direct jobs impact is relatively small.[3]

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[1] The estimate of indirect jobs is incomplete, because it only considers purchases from the project suppliers, failing to account for the suppliers' own purchases and consequent employment stimulation.

[2] Induced jobs are inferred from the Rand study, which provides estimates only of total, direct, and indirect job creation. Induced jobs, therefore, are calculated as [total-(direct + indirect)]. To the extent that indirect jobs have been underestimated, the inferred number of induced jobs shown above is overestimated.

[3] Commerce Department predictions of the total employment impact of LPW are as high as 555,000. Given a projected direct effect of 126,500 jobs, the employment multiplier is a substantial 4.35.

As mentioned above, the Rand study mixes modes of analysis. Direct, on-site employment is measured directly from EDA project data. Then, a national input-output table is consulted to obtain an estimate of the sales which the direct project work would stimulate in the supplying industries. This estimate of "indirect" sales (output) effect is converted to an indirect employment estimate using marginal employment/sales ratios.[1] Total employment is then derived from a national quarterly econometric model similar in spirit to that used by Johnson and Tomola.

The Abt study also relies on project data to derive direct employment impact. Here, however, the program under study is the Job Opportunities (Title X) Program of PWEDA, which funded more labor-intensive, shorter-term projects coming to be known as "soft" public works. Like Johnson and Tomola, Abt does not measure indirect employment separately. Rather, it estimates total impact, with "non-direct" employment (indirect + induced) being computed as a residual. However, the model used was not a dynamic (quarterly) econometric model, but the static fixed-coefficient Multi-Regional Input-Ouput (MRIO) system developed by Polenske.[2] The spatial and sectoral detail of this model is purchased at a cost--lack of dynamism, average instead of marginal impact estimates, and no sensitivity to the time path of effects. Moreover, the MRIO output multipliers must be transformed into employment multipliers, applying average rather than marginal output-to-labor ratios for each sector.

Abt estimates that the total Title X expenditure of $758 million has created 54,601 direct jobs. The total job creation forecast is 73,585, implying a national employment multiplier of 1.35. The gross government cost

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[1] Actually, such change data were available only for manufacturing suppliers. For non-manufacturing supply industries, the ratio in a single past year ("average indirect impact") is used instead.

[2] This model provides the user with estimated output multiplier for each industrial sector for each state (or aggregation of states). Because the individual state models are linked through their trade with one another, this model allows the analyst to answer questions about the location of the impacts which no other model can answer in such detail. See Karen R. Polenske and Denise D. Pasquale, "Output, Income and Employment Input-Output Multipliers," Department of Urban Studies and Planning, MIT, April 14, 1977.

per direct job is $13,880, but reduces to $10,300 per total job created.[1] If governments do not respend returns and there is substantial local substitution, job creation impact is reduced to 35,837 direct jobs, 43,381 total jobs and an implicit employment multiplier of 1.21.

Clearly, the estimates of these three studies differ greatly, as illustrated below.

Table 3

	Job Creation Effects per $1 Billion Expenditure		
Job Creation (person-year)	PSE Johnson and Tomola	Title X, Abt	Public Works, Rand (Office Building Construction)
Direct	111,100	72,040	33,280
Indirect	50,800	25,048	10,161
Induced			87,559
Total	161,900	97,088	131,000
Multiplier	1.46	1.35	3.94

In examining these estimates, it is difficult to believe that the wide differences are attributable solely to the differences in the programs being addressed. The relative scale of the direct job estimates does make sense, PSE being very labor-intensive, public works being heavily capital-intensive, and Title X falling in between (69 percent labor-intensive according to Abt). After that, however, differences become counter-intuitive. Recalling that the induced effects of an investment are unlikely to vary greatly across programs, one would expect Title X to generate more indirect and induced jobs than PSE. Thus, in relation to Johnson and Tomola,

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[1] Note that Abt also calculates that the gross government cost of $758 million reduces to a net cost of only $449 million after increased tax returns and decreased transfers are calculated. This 41 percent drop in cost reduces the net cost per total job to only $6,100.

the Abt estimates appear low. In contrast, the Rand estimates seem somewhat high. We know that Rand has somewhat underestimated indirect impact, thus somewhat inflating the induced residual estimate. However, when the estimates of "non-direct" impact for public works are twice as high as for PSE, four times as high as for Title X, one is struck with an impression of inconsistency. Relative to one another, the Abt projections seem low, the Rand projections seem high.

The point is that these three studies use different models and make different technical assumptions in arriving at their estimates. Because of this, they should not be considered strictly comparable.

This point is further illustrated by a final example. In a widely-quoted document released several years ago, the Congressional Budget Office combined simulation analysis (using one or another of the large-scale national econometric models) with a string of crude assumptions about costs per job to produce a set of employment impact estimates for four types of federal stimulus: accelerated public works, public service employment, counter-cyclical revenue sharing, and general tax cuts or across-the-board government expenditure increases. (See Table 4 below.) Since quarterly models were used, it was possible to simulate impacts over time; the basic results were displayed for "after 12 months" and "after 24 months." By introducing still other ad hoc assumptions, the CBO was also able to make "guesstimates" about the likely reduction in the unemployment rate and the net federal budget cost of each $1 billion worth of program.

The methods and assumptions used to obtain these estimates are partially laid out in an appendix to the CBO report. It must be said that they are extremely arbitrary. Both the Rand and Abt estimates were pegged to actual project data provided by EDA. The CBO estimates are much less concretely based. The PSE "low end" impact is taken more or less at face value from the Johnson-Tomola study. However, other estimated impacts are based on assumed costs per job, e.g., PSE cost is set at $8,000 per slot, with this figure then divided into $1 billion to obtain the "optimistic" impact estimate. Ranges of estimates are provided, based upon conservative assumptions about budget substitution (displacement), the proportion of federal revenue-sharing grants that go to wages versus administration, etc. Public works impacts is simulated by experimenting with the contract

Table 4

Job Creation Effects Per $1 Billion of Federal Spending			
	Initial Job-Creation Impact (000)	Impact After 24 Months (000)	Implicit Employment Multiplier
Public Service Employment	85-125	90-150	1.13-1.20
Countercyclical Revenue Sharing	40-77	72-100	1.30-1.80
Accelerated Public Works	16-46	64-80	1.74-4.00
Tax Cuts	8-15 (after 1st quarter)	30-40	Not applicable
Government Purchases	20-50 (after 1st quarter)	60-80	Not applicable

construction sector of the Wharton Model, even though other sectors of the economy are directly affected by such a program, as well.

All in all, the exercise--while useful--is the least concrete of those we have reviewed. In particular, although its intention was to permit interprogram (policy "mix") comparisons, we doubt that such comparisons are valid, given the differing eclectic methods by which the various estimates were obtained.

6.2 Subnational Multipliers

In general, the smaller the economic area under study the smaller will be the multiplier. This is due to the fact that smaller areas have more spending leakages from the new injection of funds.[1] There is no paucity of estimates (and income) multipliers for subnational economies within the U.S., although review of the available studies shows that there is little concensus

[1] Actually, this could be empirically verified through a case study as it is possible, for example, that a large city is more economically self-sufficient than the remainder of the state or SMSA in which it is included.

on the methodology to be employed in making the estimates. The magnitude of the estimates that have been cited in news articles as well as professional journals show a wide range. For example, a recent article in the New York Times indicated that the $1.5 billion spent in NYC by tourists each year made tourism a $4.5 billion industry in the city. The President of the Convention Visitor's Bureau then went on to say that this is "probably conservative," since many economists believe each dollar spent by visitors is turned over four or five times (not three).[1] Another interesting example is provided by a member of the Chamber of Commerce in Carlinville, Indiana who asserted that each dollar brought into Carlinville by the new Exxon Corporation facility creates an additional $2.50 in income in his city.[2] Where do these subnational quoted estimates come from, and what is their range and credibility? This section attempts to address such questions.

An oft-made mistake in this field is that many researchers and public speakers equate income and employment multipliers. Under certain conditions employment multipliers may be identical to income multipliers. However, it is likely that changes in the price, wage and productivity levels, income distribution and occurrence of changing returns will increase output more (or less) proportionately than employment. Moreover, the data supplied in most research reports which offer income multipliers are not sufficient to make a reliable transformation to employment multipliers. Therefore, we have chosen to exclude from the section studies which solely address income multipliers. The following examples illustrate the wide range of estimates and the techniques applied to local areas.

Eliahu Romanoff's study of the impact of housing renewal on the Lowell, Massachusetts SMSA and the surrounding areas offers some detailed estimates of both income and employment multipliers which would result from a given expenditure. He breaks down the impact into 22 different sectors of the local economy and provides employment estimates for all of them. He further breaks down estimates by types of projects for housing renewal.

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[1] Murray Schumach, "New York's Tourist Boom Bringing $4.5 Billion a Year," New York Times, November 22, 1977.

[2] Cited in Stephan Michaelson, "On Assessing Economic Impact: The Multiplier," Center for Community Economic Development, Newsletter, October-1976, Cambridge, Massachusetts.

The employment multipliers range from about 1.5 to a high of 7.0. Unfortunately, there is no explanation of the estimation technique. Careful reading allows a guess that input-output tables were utilized, although fully localized coefficients are still unavailable for even the state of Massachusetts, let alone the Lowell SMSA.[1]

In a famous work that publicized the economic base approach to analyzing employment multipliers, Charles M. Tiebout estimated the multiplier for any new investment at about 1.05 for a small city (population 15,000), at 1.10 for a larger close-by city (Evanston, Illinois, population 80,000).[2] Following on Tiebout's methodology, Ian Terner conducted a study of the economic impact of a military base on the surrounding area. In the modest-sized town of Ayer, Massachusetts, Terner found an employment multiplier of 1.2, quite close to those estimates developed by Tiebout in the earlier study.[3]

Partially in response to the increasing criticism of the economic base and related methods of estimating employment multipliers, Mathur and Rosen studied employment multipliers in the Cleveland SMSA for the 1961-66 period utilizing a series of industry regression equations.[4] Their estimate shows an additional 0.8 job for each new job in the SMSA.

Such studies establish a wide range of employment multiplier estimates from 1.05 to 7.00. While there is no real concensus, available evidence suggests that local employment multipliers will normally fall in the

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[1] Eliahu Romanoff, "Regional Impact of Investment in Housing Renewal, Project Multipliers, and Unemployment," Discussion Paper No. 71-2, Regional Science Research Center, Cambridge, Massachusetts, 1971.

[2] Charles M. Tiebout, op. cit.

[3] Ian D. Turner, The Economic Impact of a Military Installation on the Surrounding Area, Federal Reserve Bank of Boston, No. 30, 1965.

[4] V. Mathur and H. Rosen, "Regional Employment Multiplier: A New Approach," Land Economics, February, 1974.

lower portion of the range.[1] Two recent works, two of few which provide a reasonably complete documentation of methodology, confirm this generalization.

The aforementioned Rand study estimated local multipliers for specific types of public works projects. Rand used an eight-sector Labor Market Area (LMA) employment model with three major types of inputs necessary to use the model: national linkage variables, on-site public works employment and labor market area characteristics. They then made estimates for three types of projects: sewer plants, flood protection and federal government buildings.

The estimates are calculated for nine medium anad large cities and vary from a low of 1.08 in San Diego to a high of 1.30 in Sioux City, South Dakota. These values imply that each on-site job created by the direct government expenditure on the three types of projects used in the estimation creates an additional .08-.30 additional jobs within the area. The not unexpected implication is that the indirect and induced employment effects of the government expenditures in specific localities are felt principally in areas other than the local project area.

Instead of stopping here, Rand then goes on to compare its estimates with other works to show that "they compare favorably with similar estimates from other independent studies."[2] The problem is that they cite a study by John Mattila[3] which estimated income multipliers of a magnitude similar to the employment multipliers estimated by Rand. As we indicated earlier, these two multipliers are not necessarily of the same magnitude nor calculated with the same set of analytical tools.

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[1] Remember that the size of the multiplier is not necessarily the critical factor in determining total employment impact. Other factors being equal, an investment creating high direct employment will have as smaller multiplier than one which creates little direct employment. However, the authors cited above do not consider these factors.

[2] Vernez et al., op. cit., p. 161.

[3] John M. Mattila, "A Metropolitan Income Determination Model and the Estimation of Metropolitan Income Multipliers," Journal of Regional Sciences, Vol. 13, No. 1, 1973, pp. 1-16.

A final project of major importance is Karen Polenske's continuing work with the Multi-Regional Input-Output Model (MRIO).[1] The MRIO model is one of the only operational economic models of the U.S. which is capable of identifying the location of multiplier effects associated with any increase in final demand. The initial stimulus can be "national," i.e., assumed to be evenly distributed over all regions, or it can be allowed to be region-specific. And the resulting employment impacts within each region can be summed to generate a national impact. Moreover, both stimulus (new final demand) and response (additional employment) can be broken down by industry. The assumptions needed to generate such detailed output (e.g., fixed technical coefficients) are definitely restrictive, and impart unknown biases to the forecasts/simulations. Polenske points out, however, that the large-scale econometric models also suffer from serious (although qualitatively different) methodological difficulties, even in forecasting employment in a single region, and that no multi-region econometric model allowing for interregional trade is yet operational.

In the current version of MRIO, there are 51 regions (the 50 states plus the District of Columbia), 79 industries, and labor. From the inverses of the state and interstate trading matrices ('closed" with respect to household consumption and labor as the 80th "industry," i.e., allowing consumption to respond to changes in the level of income), Polenske extracts industry and region-specific output multipliers per $1 billion of new industry-specific final demand. These output multipliers ($\frac{\Delta \text{ output}}{\Delta \text{ final demand}}$) are then multiplied by industry-and region-specific average employment-to-output ratios (where average is assumed to be equal to marginal productivity), thus obtaining what the BLS likes to call employment "factors," i.e., $\frac{\Delta \text{ employment}}{\Delta \text{ final demand}}$. These factors indicate the extra employment likely to be associated with a $1 billion increase in final demand.

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[1] See, for example, Karen R. Polenske and Ruth E. Rowan, "Multiregional Multipliers for Massachusetts and New England," presented at the Seventh Northeast Regional Science Association and First Canadian Regional Science Association Meeting, May 29, 1977.

In a recent paper, previously cited, Polenske and Rowan aggregate the 51-region model into a three-region system, consisting of Massachusetts, the rest of New England, and the rest of the U.S. As a result of a $1 billion increase in final demand for lumber and wood products manufactured in Massachusetts, not only that industry (in Massachusetts) but also supplying industries, workers, and households in all three regions will be affected. Moreover, interregional trade in goods and services will take place to meet orders. When the initial impulse has fully worked itself out, the total job creation will be an estimated 161,952 person-years. However, only 17,246 of those jobs, or 11 percent, will be located in Massachusetts. 40,740 (25 percent) will be elsewhere in New England, and 103,966 (64 percent) will be located outside of New England altogether.

In the program-specific analyses conducted by Rand, Abt and others, there was a measurable initial direct employment effect, estimated directly from project records. The employment multiplier was then expressed as the ratio of total (direct + indirect + induced) to direct employment. The Polenske system is calibrated in terms of overall final demand stimulus. Therefore, it is left to the user to measure (or assume) how many initial jobs--how much "direct" employment--is created by $1 billion of final demand. This is, of course, precisely how CBO, Johnson and Tomola, and others are able to come up with numerical forecasts. If we assume that each (say) $30,000 of new final demand creates one private sector job in the Massachusetts-based lumber and wood industry (say $15,000 for wages and $15,000 for profits, interest, rent, and depreciation on capital goods) then the initial impact would be about 33,333 jobs. Since the total impact is 161,952 jobs, the implicit national employment multiplier is 4.86. If we knew how many of those (assumed) 33,333 initial (direct) jobs were located within Massachusetts, we could also compute a Massachusetts employment multiplier. Suppose, for example, that half were located within the state. Then the Massachusetts multiplier would be (17,246/16,667) or 1.03. In any case, the arithmetic itself makes it clear why national multipliers will always be larger than subnational multipliers.

7.0 TOWARD THE ESTIMATION OF DIRECT AND MULTIPLIER EFFECTS ON LOCAL AREA EMPLOYMENT

The previous review of the technical and empirical literature makes it clear that employment estimates vary greatly. The reason is that they are so sensitive to the type of model used, the exact variables which are included and excluded, the functional forms of those variables, assumptions about the design and operations of programs, and the kinds and levels of detail in the available data.[1] Estimation is even more difficult at the local level. First, local data are most incomplete. Second, even if there were complete data, a local economy is more likely to be transformed in the short run by small changes in its economic base. Third, a program which is relatively small nationally may be a major new stimulus in some local areas, and the relative scale of the local intervention may affect impacts. Finally, the dollar and time costs of constructing and maintaining systems of local models such as the MRIO are truly prodigious.

Although confronted by such substantial obstacles, local officials, evaluators and federal policymakers nonetheless recognize the critical value of obtaining better information about the local impacts of alternative economic stimulus programs. The need is there; a reasonable and cost-effective methodology is not. It is beyond the scope of this paper to develop a detailed and more cost-effective methodology as an alternative to full-scale model building. However, since that is what is needed, we can at least try to move some distance in that direction. What follows is a sketch of a procedure that planners or evaluators might be able to use to get a better handle on the local employment impacts of particular federal programs.

The basic procedure is to define and track program expenditures (or planned budgets). A documentation (or estimation) of how much a program spends, what the expenditure buys, and where the expenditure is made will lead to reasonably accurate estimates of employment impact. In this process, however, careful attention should be paid to the two factors which are most

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[1] Thus, for example, Wiseman's own experiments with a Johnson-Tomola type model of local budget substitution or displacement cause him to conclude that even "slight modifications in the functional form and time period covered can move estimated short-run displacement rates from virtually zero to as high as 80 percent." Wiseman, "Public Employment as Fiscal Policy," op. cit., p. 86.

likely to affect local job creation--leakages from the local areas and local budget substitution.

Leakages will occur when the program funds (or spending stimulated by program funds) leave the local area of interest. The most important leakages take place when program wages and fringes are paid to persons who live outside the local area and when goods and services which are produced or provided externally are purchased by the program. In other words, the location of the expenditure is critical to determining the location of the employment impacts. In general, if the local area under study is a portion of a city, impacts will be of lesser magnitude than if the focus is a city, SMSA, state or region. Starting with a clear definition of the areas of concern will allow one to identify and distinguish "local" from "outside" activity.

Second, one must be sensitive to the possibility of local budget substitution. Studies by the Brookings Institution on General Revenue Sharing and Public Service Employment have been successful in identifying the rough scale of various types of substitution through an interview/case study format.[1] Application of the Brookings approaches should yield approximate estimates of local budget substitution and a reasonable idea of how the "freed-up" local funds are used. Federal funds which substitute for local funds will have no net employment impact. They need not be tracked. However, the "freed-up" local funds should be followed in the same manner as that described below for program funds.

Being sensitive to leakages and possible substitution, an accurate budget for the local program(s) under examination should be constructed. What one wants to know, first, is the proportion of local funds spent upon direct labor (and fringes) and the proportion spent for goods and services. The BLS Factbook for Estimating the Manpower Requirements of Federal Programs provides a method for translating a particular program expenditure into a set of demands for labor and goods and services (at the two-digit SIC level). However, use of the BLS approach means that one is applying national estimates

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[1] See Richard P. Nathan et al., op. cit., for a discussion of methodologies applied to General Revenue Sharing. The PSE study is currently in progress, and early results suggest a 20 percent labor displacement rate and 15 percent final substitution rate on PSE. See Richard P. Nathan et al., op. cit.

to local areas. This begs the question of developing more precise local indicators. Thus, the BLS Factbook is best used as a general guide to local research or when access to local program data is precluded. Whenever possible, examination of program operating and financial records, supplemented by interviews with local operators and officials, is the preferred method.

Direct employment impact will be defined by the labor expenditures of the program or project. For a PSE program, labor will consume most of the expenditures, and relevant detail (what positions are funded, at what average wage, for how long) should be obtainable from project plans and records. After adjustments for substitution and/or leakages, direct job creation is simply the product of positions funded by duration of funding, disaggregated into whatever occupational categories or wage levels are desired.

For a program such as LPW, the identification of direct job impact will be more difficult in pin down. Since LPW is a bid account program, direct jobs will be distributed among the various contractors and subcontractors awarded work on the projects. Central records are unlikely to reveal the scale and distribution of jobs. Thus, one must interview the contractors and subcontractors, or a representative sample of them, to determine or estimate direct job impact.

Detailed information on a program's non-labor expenditures is the first step towards estimating indirect employment effects. If one can determine what has been (or will be) purchased and from whom it is (to be) purchased, one can roughly trace the indirect impacts of the program. For PSE, such an enumeration ought not to be a major task. Non-labor expenditures are only a very small portion of the budget, reducing the importance of precise tracking. An adequate accounting should be forthcoming from project expenditure records and/or interviews with the local purchasing official. Moreover, the range of non-labor costs is likely to be fairly predictably focused upon retail, wholesale, transportation and selected service sectors.

Such is not the case for LPW. Again, one must construct the non-labor budget from a series of contractor and subcontractor sources. And, since purchases of goods and services will consume the majority of

the budget, a reasonable level of precision is important. Moreover, a problem is posed by the likelihood that local contractors or governments purchase some or all of their materials from wholesalers and jobbers. These wholesalers, in turn, purchase their inventories from producers who may or may not be located within the "local area." Yet, examination of contractor or government records will usually only indicate the jobber, not the producer. Since it is the point of production which will determine where employment impact occurs, one ideally must also interview the jobber.

A careful "geo-coding" of program expenditures upon goods and services could require a good deal of local legwork, even if only the principal types of non-labor purchases are explored. Yet, this data collection alone will not yield estimates of indirect employment impact. What one will have is a documentation of the amounts of money spent (or to be spent) locally and non-locally upon various categories of supply. What is missing is a means to convert new purchases into new demand for labor. One way to do this is to continue tracking the non-labor expenditures to each major local producer and to develop production functions for each. The amount of new labor by occupation would be part of the production function. One could even go a step further to identify the new demand for various materials in the producing firm, follow these to "second-round" suppliers, develop production functions for these suppliers, etc. However, this entire line of inquiry is exceedingly costly and time-consuming. If carried out completely, it would essentially produce a local input-output table, the very tool for which we are attempting to develop a cost-effective alternative.

Another approach would be to consult the BLS national input-output model for labor productivity data. Again, however, this step calls into question a principal reason for doing local impact studies. It applies national coefficients to local areas, presuming that local technologies and dependencies upon external sources of supply are identical. If possible, a different solution should be sought.

One possibility to be explored is to rely upon other, more local data sources which, through imperfect, may yield a reasonable framework within which indirect impacts may be estimated. First, the 1972 Censuses

of Manufacturing, Business, etc. provide place-specific estimates of average labor productivity (value added by industry divided by employment by industry). Application of these ratios to the local non-labor program expenditures will yield estimates of the increased demand for labor by industry. Second, it may be possible to decompose these aggregated labor demands into a bill of demands for workers in specific occupations. If, for example, the area is located in a state participating in DOL's Occupational Employment Survey program, the surveys will supply the most current statewide data on occupation by industry in manufacturing, non-manufacturing, and/or government sectors. One may be able to adjust this statewide data to the particular local area within the state by consulting area-specific industry-occupation tables from the 1970 Census.

Since such an approach has not been tested, its feasibility is questionable. In any case, it could only be applied to local areas which are covered by such data sources. Further thinking and development work on this problem is required.

The process of inquiry described thus far can be summarized in the Figure 1 below and is best illustrated with a hypothetical example. Let us assume that a large urban area, the city of Cincinnati for instance, receives a total of $10 million in countercyclical funds for PSE and LPW projects. Local interviews reveal that about 40 percent of the total federal grants, or $4 million, displace local funds which would have been spent in the absence of the grants. Of these displaced local funds, 25 percent ($1 million) is used to increase fund balances, 25 percent ($1 million) offsets tax increases which otherwise would have occurred, and 50 percent ($2 million) is expended on maintaining local services which otherwise would have been dropped.[1] The $1 million used to increase fund balances can be ignored, since it will create no new jobs in the short run. The $1 million in tax stabilization will have small induced employment effects, but, for purposes of identifying direct and indirect effects, can also be ignored. The $2 million of local funds expended on services, however, should be tracked.

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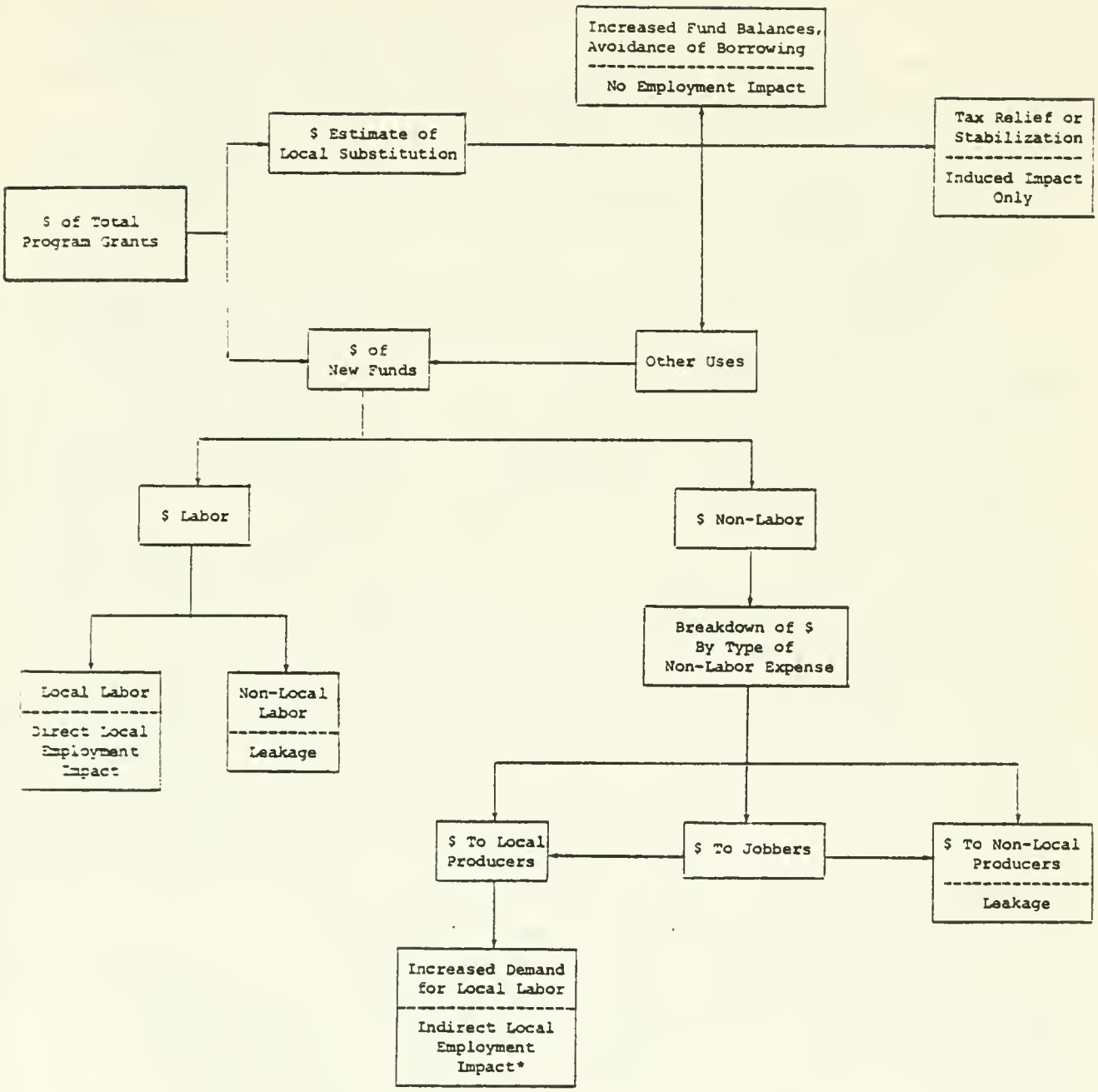
[1] Obviously, this mix of uses of only $4 million wouldn't occur at a single location. However, the example serves to illustrate several points.

Figure 1



*Excludes the increased demand among suppliers of suppliers, etc.

Thus, a total of $8 million ($6 million new federal money and $2 million "freed-up" local money) is tracked. Examination of program records and interviews with local officials and contractors reveal that $4 million is (or will be) spent on wages and fringes. However, because a non-local contractor and subcontractors have been selected to construct significant portions of the LPW projects, only roughly $3 million of the wages are estimated to be received by Cincinnati residents. Further inquiry shows that approximately 300 new direct person-years of employment are created in Cincinnati with these funds.

The remaining $4 million is used to purchase supplies, construction materials and equipment of various sorts. Interviews with the CETA prime sponsor, LPW subcontractors and jobbers suggest that only about 25 percent is (to be) purchased from local producers. The rest is supplied by producers outside of Cincinnati. The $1 million in increased local demand is estimated (by whatever means) to create roughly 30 new indirect jobs in Cincinnati.

Thus, the local job creation impact of this tracking is a total of 330 direct and indirect jobs. The original $10 million in federal grants has resulted in only a $4 million increase in spending within Cincinnati. Approximately $2 million has been deflected by local budget substitution toward uses which stimulate no direct or indirect employment. About $4 million ($1 million labor, $3 million non-labor) has leaked out of the local economy. If one assumes the same labor productivity ratios, the investment in Cincinnati has created an estimated 100 new direct and 90 indirect jobs in the suburbs and other centers of production, jobs which, barring leakages, would have accrued to Cincinnati.

The preceding example, and the initial thoughts on a local impact approach, miss several components of local job creation. Indirect job impacts are incomplete in two respects. First, no accounting has been made of the jobs created by the increased demand among suppliers of suppliers. Such an impact would be minimal in the preceding example. Nonetheless, one might explore the feasibility of developing ratio estimates of indirect jobs among project suppliers total indirect jobs, ratios which would vary according to a very rough qualitative assessment of the self-sufficiency of the local economy. Second, the indirect local employment created by economic stimulus grants to other localities

is ignored. In other words, production in Cincinnati might be stimulated by increased materials demand from Cleveland, Louisville, Detroit and other localities receiving grant funds. To address this dynamic is beyond the capabilities of any relatively simple local impact methodology.

Finally, there are induced employment impacts to be considered. Induced effects cannot be tracked and are too diffuse to merit empirical study at this level. It is not feasible to attempt to estimate induced effects separately. Instead, a rough idea might be gained by estimating a <u>total</u> local employment multiplier. The induced effect will be the residual, after estimated direct and indirect impacts are subtracted from the total (i.e., induced effect = total effect - direct effect - indirect effect).

Careful budget studies like those in Cleveland suggest that, within narrowly defined local areas, the propensity to consume locally out of newly received income may be so low as to imply local income multipliers of only 1.03 to 1.15. In larger communities with more well-developed retail and service business sectors--the portions of the economy to which most consumer dollars go--the local income multiplier will be larger, perhaps around 1.5 for a large city or metropolitan area. The associated employment multiplier will be smaller, perhaps in the range of 1.2 to 1.3.[1] Applying this multiplier range to the previous Cincinnati example, the following rough estimation would result:

$$\text{Total Employment Effect} = \begin{pmatrix} 1.1 \\ \text{to} \\ 1.3 \end{pmatrix} \quad \begin{pmatrix} \text{Direct} \\ \text{Effect} \end{pmatrix}$$

$$\text{"} = \begin{pmatrix} 1.1 \\ \text{to} \\ 1.3 \end{pmatrix} \quad \begin{pmatrix} 300 \end{pmatrix} = 330 \text{ to } 390 \text{ jobs}$$

$$\text{Induced Employment Impact} = \begin{pmatrix} 330 \\ \text{to} \\ 390 \end{pmatrix} - (300) - (30)$$

$$\text{"} = \underline{0 \text{ to } 90 \text{ jobs}}$$

Since there is likely to be some level of induced effect, however minimal, the result of this calculation would be to conclude that the local induced effect "might be as much as 90 jobs."

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[1] Note that, at this stage, such estimates are relatively arbitrary. For purposes of example, however, they are probably relatively realistic.

The purpose of this section has been to begin to explore ways in which local officials, evaluators and policymakers can arrive at rough estimates of local employment impact. Clearly, the course of action outlined herein is primitive, and a number of problems need to be addressed before it can be applied feasibly. However, if it were feasible, the extent of required local data collection would not be so immense as to prevent cost-effective local inquiry. In essence, the exercise builds several discrete data collection steps onto field approaches, such as the Brookings methodology, already being pursued.

Whether the additional information is worth the additional effort is a final question which possible users should consider. The answer will likely depend upon the programs being examined, the nature and size of the "local" area and the relative policy importance of obtaining such estimates. For example, if one were looking solely at very labor-intensive programs (like PSE), the inquiry would not be worthwhile. Most effects will be direct and are relatively easily identified. Indirect effects will be negligible and a rough local multiplier could be applied to estimate total effects (and, as a residual, induced jobs in local retail and service sectors). In contrast, the tracking of capital-intensive program impacts might be significant. Similarly, if one were only interested in a small local area (e.g., a small section of a city) one could pretty much assume that indirect and induced impacts would be minimal and, thus, only address direct effects. For a large city, SMSA, state or region, more complete inquiry might be in order. Finally, there may be important policy reasons to try to estimate local effects. For one, a rough calculation of the discrete direct, indirect and induced job creation impacts will provide a more accurate idea of the sociodemographic and sector targeting of different program mixes. Moreover, a ball-park sense of the extent of leakages out of the local area may be of significant policy importance. Whatever the reasons, the trade-off should be made. And, since in many situations the more focused attention on local job creation may be considered justifiable, further methodological work is called for.